Digitized by the Internet Archive
in 2007 with funding from
Microsoft Corporation

http://www.archive.org/details/psychologicalrev18ameruoft

5730

Vol. XVIII, No. 6 PSYCHOLOGICAL REVIEW PUBLICATIONS November, 1911

THE

Psychological Review

14

EDITED BY

JOHN B. WATSON, Johns Hopkins University
HOWARD C. WARREN, Princeton University (*Index*)
JAMES R. ANGELL, University of Chicago (*Monographs*) and
ARTHUR H. PIERCE, Smith College (*Bulletin*)

ADVISORY EDITORS

R. P. ANGIER, Yale University; MARY W. CALKINS, Wellesley College; RAYMOND DODGE, Wesleyan University; H. N. GARDINER, Smith College; JOSEPH JASTROW, University of Wisconsin; C. H. JUDD, University of Chicago; ADOLF MEYER, Johns Hopkins University; HUGO MÜNSTERBERG, Harvard University; W. B. PILLSBURY, University of Michigan; C. E. SEASHORE, University of Iowa; G. M. STRATTON, University of California; E. L. THORNDIKE, Columbia University

---

Volume XVIII., 1911.

124883
11/11/12

PUBLISHED MONTHLY BY
THE REVIEW PUBLISHING COMPANY
41 NORTH QUEEN ST., LANCASTER, PA.,
AND BALTIMORE, MD.

Agents: G. E. STECHERT & CO., London (2 Star Yard, Carey St., W. C.): Leipzig (Hospital St., 10): Paris (76 rue de Rennes). Madrid: DANIEL JORRO (Calle de la Paz, 23).

Entered as second-class matter January 21, 1904, at the post-office at Lancaster, Pa., under Act of Congress of March 3, 1879.

PRESS OF
THE NEW ERA PRINTING COMPANY
LANCASTER, PA.

CONTENTS OF VOLUME XVIII.

January

Some Properties of the Free Association Time. F. L. WELLS, 1.
Experiments on the Inhibition of Sensations. EDMUND JACOBSON, 24.
The Rôle of Pitch in Rhythm. HERBERT WOODROW, 54.
Editorial: William James. JAMES R. ANGELL, 78.

March

The Place of Movement in Consciousness. W. B. PILLSBURY, 83.
Experimental Studies of Rhythm and Time. J. E. WALLACE WALLIN, 100.
Experimental Studies in Judgment; Judgments of the Comic.
H. L. HOLLINGWORTH, 132.
A List of the Published Writings of William James, 157.
Editorial Announcement, 166.

May

A Working Hypothesis for Inner Psychophysics. RAYMOND DODGE, 167.
The Relation of the Time of a Judgment to its Accuracy. V. A. C. HENMON, 186.
Experimental Studies of Rhythm and Time (II.). J. E. WALLACE WALLIN, 202.

July

Unconscious Imitation in Handwriting. DANIEL STARCH, 223.
A Preliminary Note on the Categories of Association Reactions.
FREDERIC LYMAN WELLS, 229.
Judgments of Persuasiveness. H. L. HOLLINGWORTH, 234.
From the University of California Psychological Laboratory:
XIV. The Psychology of Change: On Some Phases of Minimal Time by Sight.
JOHN M. BREWER, 257.
XV. The Psychology of Change: How is the Perception of Movement Related to that of Succession. G. M. STRATTON, 262.

September

Imageless Thought. JAMES R. ANGELL, 295.
The System of Habits and the System of Ideas. ROBERT MACDOUGALL, 324.
From the University of California Psychological Laboratory:
XVI. Temporal and Accentual Rhythm. WARNER BROWN, 336.
XVII. Some Preferences by Boys and Girls as Shown in their Choice of Words.
M. I. STOCKTON, 347.

November

Experiments on Habit Formation. GRACE HELEN KENT, 374.
Suppression with Negative Instruction. H. S. LANGFELD, 411.
The Hygiene of Sleep. E. C. ROWE, 425.

VOL. XVIII. No. 1. January, 1911.

THE PSYCHOLOGICAL REVIEW

SOME PROPERTIES OF THE FREE ASSOCIATION TIME[1]

BY FREDERIC LYMAN WELLS, PH.D.,

Psychiatric Institute, Wards Island, N. Y., Formerly Assistant in Pathological Psychology in the McLean Hospital, Waverley, Mass.

The present discussion grows out of a body of some 12,000 observations, though all of them are not analyzed in the same detail. A general survey of a series of 100 associations with each of 25 subjects, 10 men and 15 women, makes up the first part of the report; the remainder of the experiments, in which 8 subjects, 1 man and 7 women, are concerned, are presented only with reference to specific questions of interpretation. The stimulus words of the first 2,500 observations are those of the Kent-Rosanoff standard series; grateful acknowledgment is here made of the coöperation of these writers in this portion of the study. Of the men subjects, two are physicians and the remainder nurses; the women subjects are nurses. Presentation and response were oral; in timing, the stopwatch was used.

The experiment which forms the subject of these remarks has come to be regarded from two very different points of view, and made to serve two very different purposes. In both free and controlled forms it has been the vehicle of a mass of investigations on the intimate nature of the mental process, as represented in the researches of Watt, Messer, Wreschner and others; while in different hands, the personal factors in the free association experiment have been more emphasized,

[1]From the Psychological Laboratory of the McLean Hospital.

and the attempt has been made to determine the elements of individual difference that could be measured by this means. What one gets out of this experiment may be partly a matter of technique, but it is certainly also a question of point of view. It is remarkable that the great body of work centering about the association test discontinuously with the papers of Jung and Riklin should have contained so little suggestion of those applications which they so fruitfully developed. Investigation from either of these view-points may be scientific or the reverse; it is largely a matter of temperament which view-point one finds the more interesting, though there should scarcely be the same uncertainty of which is the more directly useful. No apology seems necessary for adhering in this discussion strictly to the development of the association experiment as a method of individual psychology.

The essential routine of this familiar test is, of course, to have the operator speak a stimulus word, to which the subject is to reply with the "first thing it makes him think of," the "first word it suggests to him," "das erste Wort das klar zum Bewusstsein kömmt." In effect, this is an agreement on the part of the subject to make some connoted reply to a series of intimate questions. And in spite of all instructions to the subject about the "very *first* word that comes into your mind," etc., and the best of coöperativeness, it is naïve to suppose that we get this response from the ordinary subject save in a comparatively small number of cases. For actual progress in the interpretation of the free association test we must abandon the idea of any such objective determination of the response, and, through a study of the experimental material at all angles, endeavor to answer the query, what conditions the responses, when the instructions of the free association test are given?

It is quite necessary to study, through introspection, the subjective processes in individual associations and one can fill any number of pages describing them; but in the last analysis, the *quickness* with which the response is elicited depends essentially upon four things, which might be rated in the order of their importance about as follows: (1) The tendency of the

associations to be presented in linguistic form, or in imagery readily resolvable thereto; (2) the relative predominance of some single association; (3) the willinginess or unwillingness, from any cause, to utter the response word that presents itself; (4) such direct inhibitory effect upon all voluntary response as may be induced by any strongly emotional reaction to the stimulus word. It is understood that these factors are not mutually exclusive; any or all may frequently operate together.

Strictly speaking, not all, nor perhaps the greater number of responses in the experiment can be designated as 'free' associations. There is very apt to be more or less forced suppression, rejection, and selection. The time required to form objectively adequate associations is often considerably shorter than the actual response time. The simpler controlled associations, such as the opposites, are uniformly shorter than the uncontrolled. It requires distinctly less time to give the opposite of *slow* than to give the first thing it makes you think of. In the latter case, while the word may arouse no associations of special affective moment, there is yet likely to be some striving for the mastery among the various associations that do present themselves, with no deciding factor as in the opposites test. It is seen here how completely the association test fails as a measure of the speed of the thought processes, since it is inconceivable that a particular class of associations should, on an average, be faster in arousal than any association.

In the free association test, any response in language may satisfy the conditions of the experiment. In the controlled association tests, usually but few responses are so capable. In the first case, the subject may make any response that he pleases; in the second, only a particular response. Thus, while giving the freedom of choice over the whole range of language, the test also imposes a responsibility of choice. In the controlled association test this freedom, and consequently the difficulty of choice, is reduced to a minimum. Here the response is chosen for us, and we have only to find it. In free association we have not only to find a suitable response, but also to choose it, and consent to it.

Special difficulties that make for a lengthened free associa-

tion time have already been enumerated. All of the shortest association times may be regarded as comparatively 'free.' In a strict interpretation, we can perhaps say also that all 'free' associations are short. The term 'free' association is certainly a misnomer when applied to those cases where there has been active and voluntary suppression or 'dodging'; these associations are controlled, or 'censored,' *sit venia verbo*, as truly as those of the opposites test; only the control is variable, recognized only by the subject, and, as in Langfeld's experiments, negative rather than positive. Such association times are almost certain to be long. It is probably fair to say that in the great majority, perhaps in all, association times above the median, there has been an inhibition, rejection, a suppression of some sort; such a process is normally frequent in the experiment, though it may occur from very different causes, or be dictated by very diverse motives. It is by no means necessary that they be associated with special emotional complexes.

Nor does association with a special emotional complex necessitate a lengthened time. What is apt to cause the lengthened time in these cases is not the presence of the complex, but its suppression. If there is no motive for suppression of the ideas there is no reason why the time should be longer, unless the subject be so distracted with special interests aroused by the stimulus word that the attention required for the formulation of the response is impossible.

From what we know of the speed of the thought processes in controlled association, where all such interferences are comparatively absent, it would seem that the time of strictly free association, progressing without interference, should normally range from considerably less than 1,000 sigma up to possibly 2,000 sigma; and a time considerably in excess of this limit indicates an interfering process from any or all of the above-named sources.

From this postulate it follows, that in length and especially in the variability of the association times, one may estimate not only the liability to such interference in different classes of stimuli, but also the liability to such interference as a temperamental characteristic in different individuals.

It is not surprising to find that association time tends to be shorter in educated individuals than in less educated ones. But the individual variations are considerable, and perhaps it would be better to say the individuals with short times are more apt to be educated. Over the range of normal variability, short free association time is in but negligible degree a function of education in the individual, just as it is not a measure of the speed with which his thought processes can go on. The shortest association times I have seen have been in individuals whom, though far from illiterate, one could scarcely rank in the higher levels of education. Moreover, within a given group, the individual differences in association time may be enormously greater than the differences in education. The median association times vary a great deal more than the shortest times, which range from five to seven fifths in all subjects, and those individuals have the shortest reaction times who have the capacity for reacting to the experiment in the freest way. In some individuals nearly all reactions are comparatively free, in others but very few indeed.

To illustrate, the entire first series of experiments with twenty-five subjects may be quoted as follows:

MEN.			WOMEN.		
Order.	Median in Fifths.	Smallest Number of Steps Including 50 % of the Cases (50 % Range).	Order.	Median in Fifths.	Smallest Number of Steps Including 50 % of the Cases (50 % Range).
I.	7.7	2.7	I.	7.2	2.2
II.	7.8	2.8	II.	7.6	2.6
III.	8.0	3.5	III.	8.2	3.3
IV.	8.6	3.2	IV.	8.3	3.5
V.	8.6	3.6	V.	8.8	3.9
VI.	8.7	4.1	VI.	9.3	3.4
VII.	10.7	4.9	VII.	9.5	3.6
VIII.	10.8	5.2	VIII.	10.4	3.7
IX.	11.8	5.0	IX.	12.8	7.0
X.	13.3	5.8	X.	13.4	7.4
			XI.	14.1	8.6
			XII.	14.8	9.0
			XIII.	16.3	9.3
			XIV.	19.0	12.3
			XV.	22.0	13.6

In this table, the slowest subject takes about three times as long as the fastest, but considering that the zero point can scarcely be less than two fifths of a second, the range of indi-

vidual differences should hardly be taken as less than 4:1. The distribution of the median times is seen to be skewed toward the short end; the majority of subjects tend to approximate the limit, though others trail out to a considerable distance from it. There is no significant correspondence between the length of these times and the individual's education. The only way in which such figures can be justly interpreted is in the light of the subject's tendency to react to the experimental conditions in a decisive or unobstructed way. The association time must be regarded as a volitional rather than as an intellectual measure, indicating the tendency of the individual to make prompt choices and to act promptly upon them under the experimental conditions imposed. It measures the 'freedom' with which the subject responds.

The next point of contact between the results and this interpretation, lies in the general sex difference in association time. It has been variously observed that free association time is shorter in men than in women, but the above figures may give further suggestions as to the nature of this difference. A greater facility of response is actually found in individual women than in any of the men and the generally shorter time of the men results from their individual differences being less, and their generally closer approach to the limit.

It is not difficult to reasonably account for the way in which this result is produced. In a number of similar series of experiments[1] it has been the writer's experience that the behavior of the women under the experiments varies a great deal more than that of the men. Expecially is this true where personal or affective moments enter in the experiment to any extent. There is probably no recognized procedure in experimental psychology that involves these elements in a stronger degree than the association test. It is thus quite in harmony with previous experience that the range of individual differences is distinctly greater in the women than in the men, more so than could be accounted for in the greater number of women subjects.

In respect to this fact, as well as the generally longer times

[1]Cf. 'Sex Differences in the Tapping Test,' *Am. J. Psych.*, XX., 353–363.

of the women, it must not be overlooked that in the experiments resulting in the above generalization that women have longer association times, the experimenter has been a man. It would be by no means unnatural if these differences were much lessened, if not indeed reversed, in similar experiments made by a woman.

The sex differences noted in the median of the association time are much accentuated in the variability of the distributions given by the different subjects. This is given in the last column of the table, the 50 per cent. range. The sex differences in the range are here proportionately much greater than in the central tendency of the reaction time. In some ways, this figure has more logical significance than the median reaction time. To a number of words the subjects all react with a fair degree of promptness; it is the number of associations which are relatively obstructed, that is the important point of difference. It is here that there occurs the hesitation and indecision that results in long time and high variability, and which is seen to be so much more marked in the women than in the men.

It is more accurate to say in some women than in any of the men. For it is seen that the women fall into two distinct species here; the first eight individuals having a very small variability, below the central tendency for the men, while the remaining seven are much above it. The results of the first group are fairly commensurate with those seen in the men; the results in the second group are markedly different from those of the men. Considered as functions of temperament, the differences of experimental attitude indicated in such a grouping are not without interest.

The obvious task is to pursue these differences further, and to trace how far they may be related to the character of the responses. I have elsewhere outlined a classification[1] based upon that of Jung and Riklin which while far from final, seems to be the best that I can at present apply. Some difficulties and uncertainties of such classification have been mentioned, and certainly not overstated. It is often most

[1] 'Practice Effects in Free Association,' *Am. J. Psych.*, XXII., pp. 1–13.

difficult to decide into which of two or even more categories an association ought to be put. Indeed, this difficulty affects so directly the fundamental distinction of 'internal' and 'external' associations that it has seemed wiser to give up this distinction altogether. The reason for this, however, is pretty definite, and the general system of classification can still be preserved by making it more detailed. The 'internal' coördination (*Beiordnung*, not Jung's *Koordination*) is often indistinguishable from the 'external' coexistence, while the internal and external contrasts are absolutely continuous; it is simply a matter of the opinion as to what constitutes '*Gelaüfigkeit.*'[1] What has been done, therefore, is to include the contrasts in one group, and on the other hand to separate the *Koordination* of Jung into coördination, subordination and supraordination, since it is the first alone of these three that is so apt to be confused with coexistence. The distinction between the two is made as best it may be, but it is not nearly so well defined as others. In spite of the continuity, it has also seemed wiser to make a separate group for the more egocentric predicates. These are the most important reactions in the results from the psychodiagnostic standpoint, and show individual differences that are worth preserving.

It is well not to be too hide-bound by grammatical considerations. Little purpose can be served by a classification that calls *violet-lovely* a predicate and *violet-loveliness* a coördination. Both have the content of *Urteilsreaktionen*, and in the absence of contrary introspection should be recognized as such.

The following table will indicate the extent to which the sex differences in association time are correlated with differences in the form of the association. The table gives (1) the per cent. of associations falling into each class in all groups of subjects, (2) the median times of the associations in each class, (3) the 50 per cent. range of variability in these times.

[1]Nor is it altogether just to speak of 'mediate' responses as a restricted and minor category; as many as 50 per cent. of the responses may be 'mediate,' though falling, in their direct relation to the stimulus-word, into very different associative categories.

	Per Cent. of Frequency.		Median Time.		50 Per Cent. Range.	
	Men.	Women.	Men.	Women.	Men.	Women.
1. Failure of response	None	*None*	...	...	..	..
2. Direct egocentric	None	*None*	...	...	..	..
3. Egocentric predicate	1.4	*2.5*	18.0	*15.2*	8.0	*8.3*
4. Judgment of quality	5.0	*13.3*	12.6	*12.6*	7.5	*6.0*
5. Simple predicate	2.7	*4.9*	11.5	*12.8*	4.5	*5.8*
6. Subject-relation	1.2	*1.3*	11.3	*10.0*	6.3	*4.0*
7. Object-relation	2.1	*2.9*	9.0	*10.6*	5.0	*4.5*
8. Causality	1.1	*1.9*	11.0	*10.0*	5.0	*3.5*
9. Coördination	22.2	*13.7*	10.8	*10.9*	4.6	*4.5*
10. Subordination	4.3	*4.5*	12.2	*12.0*	5.2	*4.4*
11. Supraordination	17.7	*11.5*	10.3	*11.9*	4.3	*6.0*
12. Contrast	15.3	*15.5*	8.3	*9.1*	2.5	*3.3*
13. Coexistence	9.6	*12.3*	12.6	*13.3*	3.9	*8.6*
14. Identity	12.2	*10.3*	10.6	*13.0*	4.6	*7.8*
15. Language-motor	2.3	*2.8*	13.0	*11.6*	6.0	*5.0*
16. Word-compounding or completing	2.0	*1.7*	11.0	*11.0*	5.0	*5.0*
17. Pure sound associations	.1	*None*	10.0	...	..	..
18. Syntactic change	.8	*1.0*	11.0	*15.5*	4.0	*7.5*

The third, fourth and fifth categories, representing predicates and *Urteilsreaktionen*, are about twice as common in the women as in the men. This difference is quite consistent; the highest number reached by the men but little exceeds the average of the women. On the other hand, there is no corresponding difference in the association times, these being practically equal. The subject-relations, object-relations, and causalities perhaps occur slightly oftener in the women; the balance of the time difference is slightly in favor of the men, but the cases are throughout very few in number. A marked difference is seen in the consistently greater number of coördinations that the men assign, but there is no difference in the association time. The subordinates are practically equal both in frequency and in time. The supraordinates are again much more frequent in the men, and from here on the men begin to have distinctly shorter times. The contrasts are about equal in frequency, and it is worthy of note that in both men and women they have shorter times than any other category. The coexistences seem a little more frequent in the women, the identities in the men, the men

having shorter times in each case. In accord with other experience, the remaining categories are very scantily represented, and the times, even were the differences significant, would not influence the general results in any appreciable degree.

The shorter time of the men, therefore, is expressed in their quicker reactions in the associations of supraordination, contrast, coexistence and identity. This finding, together with the excess of the higher predicates in the women, probably harmonizes with the results of Jung and Riklin regarding the internal and external categories.

The figures of these results, more minutely analyzed, are very much against the supposition of a long or short reaction time as characteristic of an individual with a predicate, concrete, or other association type. Extremes of long and short reaction time may be found in extremes of either type, though there is little question that; in the long run, the times would average greater in those individuals who have many predicates. Altogether the most striking correlation of this nature concerns the supraordinates and the contrasts. There is strongly negative correlation between these two; a subject is apt to have many supraordinationes and few contrasts, or few supraordinates and many contrasts. The latter individuals not unnaturally have the shorter reaction times but it would be an error to infer from this that their generally shorter times are due to this abundance of contrast reactions. For in the first place, the difference between the supraordinate times and the contrast time is itself far too small to account for this difference between the groups, and secondly, the shorter times in these individuals are by no means confined to the contrasts. It seems more probable that those individuals possessing the inherent faculty of short association time, are more apt to react in this way. The correspondence, though not exceptionless, is quite consistent, being as follows:

The one markedly contrary case is the third subject among the women, though the fourteenth is also exceptional. If the subject has a short reaction time there are likely to be more contrasts than supraordinates; if a long one, more supraordinates than contrasts.

Men.			Women.		
Median Time of 100 Associations.	Number of Subordinates.	Number of Contrasts.	Median Time of 100 Associations.	Number of Subordinates.	Number of Contrasts.
7.7	6	30	7.2	4	30
7.8	17	19	7.6	9	30
8.0	20	20	8.2	37	2
8.6	3	27	8.3	7	12
8.6	6	30	8.8	7	32
8.7	7	17	9.3	1	31
10.7	29	1	9.5	2	27
10.8	30	6	10.4	6	14
11.8	33	1	12.8	18	4
13.3	26	2	13.4	5	2
			14.1	19	2
			14.8	12	1
			16.3	8	3
			19.0	14	24
			22.0	23	18

A query which suggests itself in connection with these species in the contrast reactions is whether the forms of association that are infrequent in an individual tend to have longer or shorter times than those which are more common in him. With the contrasts, those who habitually resort to them have shorter times in them than those who do not, but elsewhere it is by no means the rule. While there are individual cases of very long reaction time in associations alone in their class, no significant correlation is here shown statistically. No general rule can be formulated for the order of association times in the different categories. The contrast associations, for example, are generally shorter than the identities, but in several individuals they are longer. It does not seem possible to analyze these differences further. Beyond such generalizations as that, by and large, coördinates are likely to be shorter than coexistences, and such special phenomena as characterize single individuals, reaction time and logical category are practically independent.

There is no reason why associations of especially affective moment should not take any of the forms mentioned, from failure of response to repetition of the stimulus,[1] especially where dodging is concerned; though the most apparent ones

[1] "By heaven, he echoes me,
As if there were some monster in his thought
Too hideous to be shown." *Othello.*

naturally fall into the category of the egocentric predicates. The longer times in this category are fully accounted for by this consideration.

In the foregoing, the experiment has been discussed almost exclusively from the standpoint of central tendencies, and with reference to the difference between individuals, or groups of subjects. An adequate conception of the picture can scarcely be given without considerable attention to the experiment from the standpoint of the individual stimulus words. One must not dispense with the minuter discussion of individual reactions, unfit as such treatment may be to establish general principles. The obvious danger of such a treatment is that in the quotation of what may be especially glaring instances of *Komplexwirkung* or what not, an exaggerated or distorted idea of the experiment is apt to be given. Against sensational interpretations of the experiment it is to be hoped that the previous remarks will serve to guard; yet in any endeavor to see the forest in spite of the trees, it is well not to entirely avoid seeing the trees because of the forest.

Previous investigators have examined the relation between the reaction-time and the grammatical categories of the stimulus words. The results do not altogether agree, but there is some tendency for the abstract nouns to be longest and for the adjectives and concrete nouns to be relatively short. In the Kent-Rosanoff series there are no verbs that are not also nouns; in central tendency, the concrete nouns are shortest, the adjectives next, and the abstracts distinctly longest. The two shortest words of the hundred are *bitter* and *ocean*, each with a median reaction time of 6.9; the longest word by many steps is, not insignificantly for the psychoanalysts, *wish*. Its reaction words are as follows:

desire 36	would 32	request 18*	thinking 13	think 13
grant 22	have 10	thought 18	wanting 25	want 7*
have 65	granted 21	uncertain 23	gratified 13	happiness 34
fancy 32	granted 8	want 18	idea 32	desire 10*
money 22	true 8	comfort 21	gratify 25	hope 9

All times save the three marked with an asterisk are above the median reaction time of the subject that gave them.

The words of the entire list have been arranged in the order of their median times, and this order will perhaps have sufficient intrinsic interest to warrant its printing in full,

Order.	Word.	Median of 25 Observations.	Smallest Number of Steps Including 50 % of the Cases (50 % Range).	Order.	Word.	Median of 25 Observations.	Smallest Number of Steps Including 50 % of the Cases (50 % Range).
1.	Bitter.....	6.9	1.9	51.	Priest.....	10.5	4.2
2.	Ocean.....	6.9	2.2	52.	Bible	10.5	4.5
3.	White.....	7.2	2.5	53.	Mountain ..	10.5	4.5
4.	Dark	7.5	2.7	54.	Fruit	10.5	4.5
5.	Yellow.....	7.7	2.7	55.	Music.....	10.5	5.5
6.	Swift	7.7	2.8	56.	Rough	10.5	6.1
7.	Thirsty	7.8	2.6	57.	Thief	10.5	6.2
8.	Sour.....	7.8	3.5	58.	Boy	10.7	3.8
9.	Stove.....	7.9	3.1	59.	Deep	10.7	4.7
10.	Scissors	8.2	3.2	60.	Lion.....	10.7	4.7
11.	Black.....	8.2	3.2	61.	Heavy	10.9	5.7
12.	Girl	8.2	3.7	62.	Head	11.1	4.4
13.	Man.....	8.3	2.8	63.	Loud	11.1	4.9
14.	Eagle.....	8.4	3.4	64.	High.....	11.1	6.1
15.	Long.....	8.4	3.4	65.	Window....	11.1	6.1
16.	Blossom....	8.4	3.8	66.	Carpet.....	11.2	3.8
17.	Sweet.....	8.5	3.5	67.	Working ...	11.2	4.2
18.	Hammer ...	8.7	2.7	68.	Stem	11.2	4.2
19.	Sheep.....	8.7	2.9	69.	Hungry	11.2	5.2
20.	Blue.....	8.7	3.6	70.	Cold.....	11.2	5.2
21.	Spider	8.7	3.7	71.	Afraid	11.2	5.2
22.	Table.....	8.7	3.7	72.	Street.....	11.2	6.2
23.	Bath.....	8.7	3.8	73.	Hard	11.5	5.5
24.	Cottage....	8.7	4.8	74.	Smooth	11.5	5.5
25.	Soft.....	8.8	3.5	75.	Soldier.....	11.5	6.2
26.	Mutton	8.8	3.7	76.	Child	11.5	6.5
27.	City.....	8.9	3.5	77.	Chair	11.5	6.5
28.	River.....	9.1	3.7	78.	Butter	11.5	6.5
29.	Lamp.....	9.2	2.7	79.	Sleep	11.5	6.5
30.	Baby	9.2	4.2	80.	Memory ...	11.6	4.6
31.	Needle.....	9.2	4.2	81.	Butterfly...	11.6	4.7
32.	Bed.....	9.4	3.6	82.	Cheese.....	11.6	5.5
33.	Bread.....	9.4	3.6	83.	Foot.....	11.6	5.5
34.	Joy.....	9.4	4.2	84.	Dream.....	11.6	6.2
35.	Green.....	9.4	4.4	85.	Comfort....	11.8	5.7
36.	Whistle	9.5	3.7	86.	Quiet	12.5	6.5
37.	Woman	9.5	4.5	87.	Whiskey ...	12.5	6.5
38.	Sickness....	9.6	3.8	88.	Salt	12.5	6.5
39.	Earth.....	9.6	3.9	89.	Citizen.....	12.5	7.5
40.	Hand.....	9.7	4.2	90.	Trouble....	13.5	7.5
41.	Red	9.7	4.7	91.	Square.....	13.5	8.5
42.	Tobacco....	9.7	5.2	92.	Command ..	13.7	6.5
43.	Eating.....	9.8	3.5	93.	Anger.....	14.2	7.2
44.	King.....	9.8	4.5	94.	Religion....	14.2	8.5
45.	House	9.9	3.5	95.	Moon.....	14.2	9.2
46.	Doctor.....	9.9	4.7	96.	Slow.....	14.5	9.5
47.	Beautiful...	10.2	4.2	97.	Health.....	14.5	10.5
48.	Short	10.2	5.2	98.	Justice.....	15.5	7.7
49.	Light	10.2	5.5	99.	Stomach ...	19.2	13.2
50.	Cabbage ...	10.5	2.7	100.	Wish	20.2	12.7

Some examination was made of the classes of association in the ten longest words, but the forms of association did not seem different from those of other words; there is illustrated, however, a tendency of certain words to draw to themselves special forms of association.

Among the 2,500 observations, there were 90 of ten seconds and over in length. Terrible indeed must have been the emotional complexes stirred up in these associations if the time be directly proportional to the affective reaction. As a matter of fact, the group appears commonplace enough in everything but time, including such reactions as *fruit—apples 72, deep—ocean 51, rough—smooth 260, cabbage—leaves 65, city —town 101.* When these associations are classified, the coördination is seen to be the most common form, though there is a fair sprinkling of predicates; nothing out of the ordinary.

It is, however, significant that of these extra long responses the men gave 18 and the women 72, proportionately three times as many. The lesser freedom of the women in reacting is nowhere more strikingly evident than here. To understand these results, there is no necessity for assuming everywhere a suppression of disagreeable ideas, probable as this may be in individual cases. There need be nothing more than a perfectly commonplace difficulty in selecting a response with a sufficiently intelligent sound. The failure to do so with fair promptness calls forth a certain emotion of annoyance, which of course further increases the difficulty in responding, and this process may repeat itself indefinitely. Often by using a little 'summation,' repeating the stimulus word in a sharper tone, an entirely ordinary response is elicited with slight delay.

In six records of a series of 1,000 associations, the subjects being women nurses,[1] the following stimulus words had reaction times in every case above the median time of the subject for the series in which it occurred:

adorn	discourage	lame
alone	election	priest
amuse	enthusiasm	person
apology	escort	proverb

[1]There are seven subjects, two reacting to only half the entire list.

ashore	fault	shame
attraction	garter	sport
breast	goat	starch
caution	greeting	strange
celery	imagine	surprise
conscience	important	swear
contrast	impress	trouble
depressing	instruction	unsteady
	invitation	

There are 37 of these words, considerably more than the chance number. A scant half dozen seem to be words that one might expect to arouse intenser emotional associations in individuals generally; in a greater number, the long reaction time is related rather to the unfamiliarity of the stimulus word, while in still others no reason beyond that of chance coincidence is apparent.

Of all the words occurring, *person* seems to be that most frequently associated, either as a stimulus or as a response, with long reaction times, egocentric associations, or both. It chances that in the series it is immediately followed by the stimulus word *virtue*. Here also there are several abnormally lengthened times, the associations for both words being as follows:

person	man	14	woman	26	human	16	woman	40	face	19	woman	13
virtue	good	18	reward	14	man	8	truth	11	goodness	20	virtuous	26

This was the only general appearance of perseverative effect.

In these experiments there were also a few instances of entire failure of the response. The stimulus words in these cases were, for the women nurses, *disgrace*, *betray*, *expect*, *information*, *cravat*, *magnet*, *diagram;* for the physician, *intend*, *never*, *interest*. Some of these words have obvious 'complexual' implication, others not.

One of these subjects had a special tendency to react with the response of *word*. As this was always accompanied by an abnormally long time, and occurred in response to such stimulus words as *useless*, *husband*, *honor*, *against*, it might have been superficially set down as a sort of 'Komplexvertreter.' The subject's own account of these reactions is simply one of a failure of associations to arise, the stimulus word retaining

paramountcy in consciousness. The response of *word* thus refers to the imagery of the stimulus word itself. The subject specifically asserted that dodging did not occur in these cases, while admitting its occurrence in other associations. A similar process may bring about failure of the response, or the repetition of the stimulus word, according to the idiosyncrasy of the subject. It is impossible to assign to the emotive element the principal rôle in all such cases, unless one prefers the short cut to consistent findings of improving upon the introspection in a way that, as Titchener has phrased it, voluntarily abandons the sphere of fact for that of fiction.

The diurnal variation in the reaction time of these 1,000 word experiments is discussed in another paper.[1] Little need be added to these remarks, and to those on the Kent-Rosanoff experiment. Perhaps it might be further emphasized that the practice effect is essentially the reduction of abnormally long times and is to be expected in those individuals, who, through personal idiosyncrasy or inferior adaption to the experiment, show such lengthened times. Owing to the fact that the individual differences in reaction time as we see them at the beginning of practice are not matters of education, it seems probable that they are not themselves matters of special practice, even though such practice tends to obliterate the differences. The determinants of long or short reaction time at the beginning of special practice are given in the original nature of the individual rather than in his training.

It has not seemed worth while to minutely analyze the greater part of the associations in these longer experiments for the purposes of this report. No change in the interpretation of the Kent-Rosanoff experiments is indicated in the analyses that have been made. These, beyond those above mentioned, are directed mainly towards throwing further light on the probable error of the free association times of the same word in the same individual, and the ever recurring question of the time as related to the emotional reaction.

First, regarding the question of the probable error.

[1] *Am. J. Psych.*, XXII., pp. 1–13.

While practice shortens the central tendencies of the times, it does not bring down their lower limit very much; the fastest four or five reactions for one series are apt to lie between 5 and 7 fifths for all subjects during all experiments. For this reason, the few shortest words in any series may be regarded as comparatively immune from any decrease in reaction time by practice. The three stimulus words having the shortest times in each individual series[1] were therefore made up into special lists, these being different of course, for each subject. Each subject was then given his own series, and there could be observed the tendency of these shortest times to remain consistently so.

When the association time of each individual stimulus word, as given in the first instance, is compared with its association time at the second presentation, the mean variations average as follows:

Subject.....	Black	Brown	Red	Orange	Green	Blue	
M.V.....	.61	.43	2.0	1.6	1.2	.49	fifths of a second.

These figures indicate that where the process of responding is found to be free and unencumbered by hindrances, emotional or otherwise, the reliability of the individual measure is between one tenth and four tenths of a second for each of the different subjects. In these cases, then, the freedom of reaction seems to be a quality fairly inherent in the word, so far as the individual subject is concerned. This might not be so markedly the case with words having longer times, but it is impossible to test them directly. In order to obviate the memory factor, there must be much experimental material intervening between repetitions, and where this is the case, the longer times undergo an independent reduction by practice. The only method is to determine if the longer words maintain a similar relative position. As appears below, they show some tendency to do this; the most precise determination seems scarcely worth the labor it would involve.

From each series, three words were similarly selected whose times had fallen at the mode, and the three longest reactions

[1]Excluding the first two, and the shortest series for each subject, which were repeated separately. From the twentieth series only two words are taken.

of each series were also gathered into a separate group. The association times in these individually selected series are as follows:

Subject.	Black.		Brown.		Red.		Orange.		Green.		Blue.	
Series.	Med.	50 % R.	Med.	50 % R.	Med.	50 % R.	Med.	50 % R.	Med.	50 % R.	Med.	50 % R.
Shortest..	6.5	1.2	6.0	2.0	8.4	2.6	9.2	2.0	7.9	2.3	5.3	.9
Modal ...	Not given		7.5	2.4	11.0	4.4	10.7	3.4	9.5	3.0	7.3	1.5
Longest ..	9.1	2.7	12.0	5.0	16.0	7.6	11.0	10.0	15.0	10.0	8.1	2.8

As can be seen, the shortest stimulus words remain definitely shortest; in three subjects the longest approach very close to the mode, while in the others they remain a considerable distance from it. In some ways, this difference in the ability to reduce the longest times is a more significant datum than the general practice effect itself. It is most strikingly seen in Subjects ORANGE and GREEN, the former of whom has brought the longest reactions practically to the level of the modals, while in the latter a marked difference still remains. The much greater variability in the longest times is a striking, though not unnatural feature. When the associations in these series are classified there is a general tendency for the contrasts to decrease in number and for the predicates to increase, in the longest as compared with the shortest series.

To each subject was also repeated the series which had had the shortest median time in the practice experiment, the median times in each case being:

Subject.	Black.		Brown.		Red.		Orange.		Green.		Blue.	
Series.	Med.	50 % R.	Med.	50 % R.	Med.	50 % R.	Med.	50 % R.	Med.	50 % R.	Med.	50 % R.
Shortest..	7.0	1.8	8.2	3.3	9.1	2.9	8.7	3.0	9.0	3.0	6.3	1.4
Repeated .	7.1	1.6	6.7	2.0	8.4	3.0	8.1	2.6	8.0	3.0	6.3	1.5

Except in the first and last subjects, who began nearest the limit of quickness, the time in the repeated series is noticeably shorter than in the original, illustrating the practice effect that intervenes.

In the experiments just described, as well as in a number

of additional ones,[1] the attempt was made with four of the women subjects to further observe the relations of the reaction time to the emotional response. The method was one that was developed in the laboratory for purposes connected with the 'psychogalvanic reflex.' It consisted, as elsewhere described, in causing the subject to assign the emotional response to one of four grades, which may be termed A, B, C and F, according to its greater or lesser emotional value. This procedure has undergone extended test with subjects more skilled in introspection. With the present group of subjects it naturally does not attain the reliability that it would have in the case of the more practiced introspectionists. There is every external indication that the main sources of error that a practiced introspectionist observes, enter in a greater degree into these results also. Emotional reactions of an elevated character are graded too high, those of a baser character too low, and there may be dodging in the assignment of the grades.

In certain instances, the grade assigned may indeed be very different from that which one would, on the external evidence, have estimated it to be. High grades may attach to short times associations of relatively superficial appearance, as the grade A to *hot—cold 6*, *tap—water 6*, *mountain—hill 7*, *needle—sew 7*, *hay—grass 7*, *luck—good 7;* and the grade F may attach to long-timed associations where an emotional implication lies nearer, as *flirt—naughty 24*, or *kiss—osculation 28*. It is possible to assume *ohne weiteres* that the introspective data are faulty in such cases, though it is scarcely scientific to do so. One must be guided by such introspective data as are indirectly obtainable, as well as by external acquaintance with the individual subjects.

In all, there are available for study 1,388 observations by this method, an approximately equal number with each of the four subjects. If the length of the association times is directly correlated with the intensity of the emotional response, then the central tendencies of the times attaching to each of the grades should decrease in length as the grades decrease in intensity. The figures are given in the following table:

[1]Including a translation of Jung's series beginning *Kopf, grün*, etc.

EMOTIONAL GRADES.

Subject.	A.		B.		C.		F.	
	Med.	50 % R.	Med.	50 % R.	Med.	50 % R.	Med.	50 % R.
Brown ...	13.0	6.6	11.0	4.5	8.2	3.3	7.6	3.6
Red	11.4	5.4	10.0	4.2	10.3	3.9	11.3	7.2
Orange ...	11.7	3.8	10.5	3.2	10.9	3.5	11.2	4.0
Green	10.3	4.6	9.7	3.7	9.3	4.2	10.9	5.0

The situation is seen to be complex. Only the first subject shows the progressive decrease of the times with the grades. In the first three subjects the A-grades have indeed the longest times, and in the fourth subject the next to longest; but in every subject except the first one, the times are short in the B-grades and then tend to increase to the end. In the fourth subject, the F's actually have a longer median time than the A's. It is seen that we cannot regard the emotional intensity as the sole important factor in lengthening the time, unless we are prepared to consider the grading not only unreliable but very ingeniously falsified. On the other hand, certain observations made with more practiced subjects have shown a somewhat (not greatly) closer correspondence between high grade and long time. But in view of the mutual agreement of the present results, can we attribute this entirely to more accurate introspection?

The matter shapes itself somewhat as follows: Probably no one could seriously assert that increased emotional reaction is the sole cause of lengthened association time, though it need not be questioned that it is an important contributing cause. The principal other factors are the failure to find a response that is intellectually satisfying, and a failure of predominance of any one among the associations presented. These situations, while they need not be devoid of emotional content, can scarcely have that of the associations touching on genuine emotional complexes. Other things being equal, the difficulty of intellectual satisfaction is certain to be greater in less educated than in highly educated individuals. In the present group of subjects, it therefore tends to contribute a much greater share to the lengthened association times which therefore tend to be graded low in emotional value.

The results of all the subjects except BROWN strongly indicate the presence of two such independent factors in lengthening the time; the times of the A-grades tend to form a distinct species with reference to those of the other grades. If the remaining grades, where we deal only with lesser orders of emotional intensity, were chaotic, then this interpretation would suffice, but there appears a curious tendency of the intellectual difficulty to be inversely related to the emotional reaction. No constant error of introspection seems to account for this relationship. Possibly one might here regard the affective moment as similar in action to drugs which in smaller qualities acted as stimuli, before in larger amounts they produced inhibitory effects. While beyond a certain point the intensity of the affective moment tends to volitional paralysis, up to a certain point it may easily act as a spring upon the response, and where it is conspicuously lacking, the time of the response may be lengthened through its sheer indifference.

In this connection it is worth while to mention the distribution of the different grades in the three series made up of the stimulus words which had had respectively the shortest, modal and longest reaction times. The total number of A-grades in the longest series is 42, in the modal series 40, in the shortest series 22. The number of F-grades is in the longest series 104, in the modal 79, in the shortest 75. The longest series tends to have its words graded in the extreme positions; every subject shows a drop in frequency at the C-grade, while F-grade is most numerous of all. It thus seems that there are two sorts of 'too long' reactions, one of which tends to be graded quite high, and the other quite low. This is also seen in the detailed distributions of the reaction times for the different grades; in two subjects, more reactions of 30 fifths and over are graded F than are assigned to all the other grades put together. It may be added that the number of A grades is throughout considerably greater than has been assigned by better practiced observers.

The results given and the conclusions suggested in the foregoing observations may be briefly reviewed. In respect

to sex differences in association time, the women cover a much wider range, slightly shorter times being given by individual women, as well as very much longer ones. The central tendency of the times is longer in the women. The same phenomenon is exaggerated in the variability of the times in the single individual. The women fall about equally into two distinct species, one of which is slightly more constant than the men, the other considerably less so. In respect to form of association, the women give some twice as many *Urteilsreaktionen*, and considerably fewer coördinations and supraordinations. The generally shorter times of the men are expressed in prompter reaction in the categories of supraordination, contrast, coexistence and identity, perhaps also in the more superficial predicates. The most clear cut relation of association type to time is independent of sex difference and appears in a negative relationship between the number of contrasts and the number of supraordinates. The individual with the longer time is likely to favor the supraordinates rather than the contrasts. In the Kent-Rosanoff experiments, there was evident no general tendency of the more frequent classes of associations to be shorter or longer than the less frequent ones. Further, the 90 longest associations of the experiments showed no marked peculiarities in association form. Throughout, there were few phenomena of perseveration, and indeed all the lower forms of association were relatively infrequent. The probable error of an absolutely free association time is rarely greater than 5 per cent. of the time itself.

The endeavor to systematically correlate the association times with the introspection of the emotion indicates that while associations of special emotional coloring are apt to have on the whole a longer time than the entire body of the associations, yet there are factors quite independent of the emotional response which may result in reaction times as long as or longer than those where the emotional factor is principally concerned. This is true whether the subject is practiced in introspection or not. In all cases, the variability of the times attaching to any grade of emotional re-

action is such as to render the reliability of the time as an index of the emotion, in individual observations, practically *nil.*

The free association time is exactly what its name implies, a measure of associative freedom. This involves a freedom of presentation, of selection, of expression. We obtain uniformly short association times in those individuals in whom all these processes are comparatively free. Where longer times are found, obstruction of some sort is indicated. We have traced some of the characteristics of this obstruction; of its range of variability, how under the conditions it was more marked in women than in men, especially seen in certain classes of associations, and in special stimulus words. This is as far as the unsupported association time will carry one. It remains then to become acquainted with the nature of the obstruction. Where it is found, it must not be incontinently set down to emotional factors, but the rôles of suppression, distraction, indecision, and the like, must be carefully compared and weighed by the best available means. Then only do the differences in association time assume their complete significance for the test, which is the most searching of all psychological experiments, and gives more coercively than all others, a paradigma of the subject's mental habits.

EXPERIMENTS ON THE INHIBITION OF SENSATIONS[1]

BY EDMUND JACOBSON

§1. *Introduction.*—The inquiry as to how simultaneous sensations affect each other has an obviously fundamental importance. With apposite technique Heymans has shown that threshold sensations of color, taste, sound, pressure and light may be totally suppressed by other strong sensations respectively of the same quality. He has found also that the faint sensations caused by the ticking of a watch may be inhibited by applying a faradizing current to the fingers. On detailed observations he has based several laws, the first being that the inhibitory power of a sensation increases in proportion to its intensity, that increase being measured in terms of the stimuli to the sensations which it is able to suppress.[2]

Heymans assumes without comment that when faint sensations are inhibited, this is equivalent to a diminution of their intensity. In this he is simply accepting the word of his subject, who with his attention voluntarily directed to the sense-organ finds them weakened or absent. Were one to suppose that the faint sensation is present in the subject's consciousness, but that he is unable to get at it, one would be

[1]From the Harvard Psychological Laboratory; being a revision of an experimental chapter of a thesis on 'Inhibition' submitted in partial fulfillment of the requirements for the degree of Doctor of Philosophy at Harvard University, June, 1910.

[2]For these laws he claims a far-reaching character, applying them for the explanation of the basis of sensational thresholds. In addition he calls attention to other experiments—certain ones made by himself and certain ones made by others—the results of which he interprets as showing that sensations of difference ('*Unterschiedsempfindungen*') both weak and strong, can be partially or totally suppressed by ordinary sensations of vision or the like or by other sensations of difference. This being so, he sees in it the explanation of Weber's law. Our present concern lies only with the first law. 'Untersuchungen über psychische Hemmung,' *Zeitschr. f. Psychol.*, 21, 1899, 321 f.; *ibid.*, 26, 1901, 305 f.; *ibid.*, 34, 1904, 15 f.

implicitly asserting the existence of an unconscious sensation,—which is an evident contradiction. The intensity of a sensation is precisely that in it which tends to increase if, for example, the corresponding stimulus is increased; and what Heymans finds is that he may increase the stimulus and yet the faint sensation will fail to appear in consciousness if also he sufficiently increases the stimulus to the stronger inhibiting sensation. Hence it is with justice that he assumes that the strong sensation decreases the intensity of the faint one.

Granting, however, that liminal sensations may be obliterated by other but powerful sensations either of like or of unlike quality, for instance, that one produced by a few hundred milligrams of pressure might vanish if a three hundred gram weight were applied to the skin near by—further research seems necessary in order with surety to warrant such assertions as that the three hundred gram weight would occasion an appreciable loss in a sensation far above the limen in strength, for instance, in one produced by a 20-gram weight. For it is easily conceivable that the latter might be powerful enough effectively to resist even partial inhibition; and even if it did suffer a diminution equivalent to a few hundred milligrams, we might anticipate that this loss would not be noticeable, according to the well-known data that center about Weber's law. And the whole situation is further complicated by the fact that other observers than Heymans have found that distractives may sometimes fail of their purpose and work augmentor effects instead of inhibitory. Hence it seems interesting to inquire how moderately strong sensations would be affected, if at all, by other simultaneous sensations.

First of all the problem arises as to some means for measuring the sensations in the subject's mind. At the same time, the well-known historical controversies about whether increased attention brings increased intensity to sensation make it clear that all attempts to come to definite solutions through direct appeals to the introspection of the subject would be likely to prove futile. Accordingly, it is necessary

to find some other means for measuring changes in intensity, some relatively objective scale or standard. Such an objective scale or standard is furnished in regard to any given sensation by comparing it with others subsequently or previously given and determining how many times out of ten it is judged stronger than they. If, for instance, a pressure sensation produced by 20 grams is to be used, then compare it with sensations produced by weights of 12, 15, 17, 18, 21, 23, 25, 27 and the like. In this manner one can find a rough scale in which the 20-gram weight will be judged heavier, say 10, 9, 6, 5, 4, 2, 1, 0 times out of ten than the other weights. One may now choose 2 weights C' and C such that the one is somewhat heavier than 20 gr. and the other somewhat lighter, and hence that the 20-gr. weight on the average is judged heavier than C' from 1 to 3 times out of ten and heavier than C from 6 to 9 times out of ten.[1] Then a sound may be produced simultaneously with the sensation occasioned by the application of C' or C, and the number of times out of ten that the 20-gr. weight (given always without the sound) is now judged heavier than C' and C may be determined. If the 20-gr. weight is judged heavier than C' more times when the sound is applied with the latter weight than when not, one would have some reason to believe that the sound diminished the sensation of C'. Yet this same result might be consistent with the very different assumption that the sound simply confused the judgment and rendered discrimination less exact. If 20 gr. is judged heavier than C' only two times out of ten, then any disturbance of the exactitude of judgment might conceivably lead to an average increase to three or four times out of ten. But suppose that in addition to showing that the 20-gr. weight is judged heavier than C' with the sound more often than without, the statistics showed also that it was judged heavier than C more often when the sound occurred simultaneously with C than when not: then the result would be unequivocal. For the figure for C is already about eight out of ten, and there would be no reason for sup-

[1]The standard weight which we arbitrarily name here as 20 gr. may of course have other denominations. In our later discussions we shall call it S, leaving C to stand for the lighter weight that is compared with S, and C' for the heavier weight.

posing that disturbance of discrimination would increase this number on the average still nearer to ten. Far more would it be to be expected that poorer discrimination would tend to make the subject unable to call 20 gr. heavier than *C* so many as eight times out of ten; or else that it would increase the mean variation about eight. But if not alone the figure for *C'* but also that for *C* is increased, then there could remain no reasonable doubt that since the sound makes the heavier weight reported lighter and the lighter weight even lighter still than before, we might infer that the sound in some way diminishes the weight sensation. This method of inquiry lies at the basis of Series 1.

Series 1.

Inhibition of Pressure Sensations by Sound Sensations.

§2. *Apparatus.*—Sound sensations of moderately strong intensity were produced by means of an electrical buzzer always placed 30 inches from the left ear of the subject, whose head was secured by means of a head-rest. The sound consisted essentially of loud clicks in so quick succession as almost to be fused. The sound varied somewhat from day to day, particularly because of variations in the current, but it was sufficiently satisfactory for the present purpose.

Pressure sensations were produced by means of the instrument used by Kobylecki and originally invented in a simpler form by Stratton. The former has given a full description of it in *Wundt's Psychologische Studien*, Vol. 1, and it will suffice here to give a simple explanatory sketch of the machine as it was adapted for the purposes of Series 1 (cf. Fig. 1). The subject sat facing the machine with elbow supported, his right hand placed palm downward on the rest indicated under *T*. As will be seen, only two levers, *AB* and *DC*, are used, and these are evenly balanced and move freely. At the point *A* there projects downward a rounded stick of light wood. The lower end of *T* did not come in contact with the hand, but rested on a cork disc about 15 mm. in diameter, the position of which was usually somewhere between the knuckles of the middle and fore-finger. Other locations,

also, were used at times. It will be apparent from the sketch that a weight placed on the pan *R* will cause *T* to move downwards and thus communicate pressure to the cork disc on the hand. It is advisable, however, to have the lower end of *T* always in contact with the cork disc, else the application of the weight to the pan *R* will cause *T* to fall suddenly and thus give a sensation of impact instead of mere pressure. The screw *K* can be used for this purpose but it works very

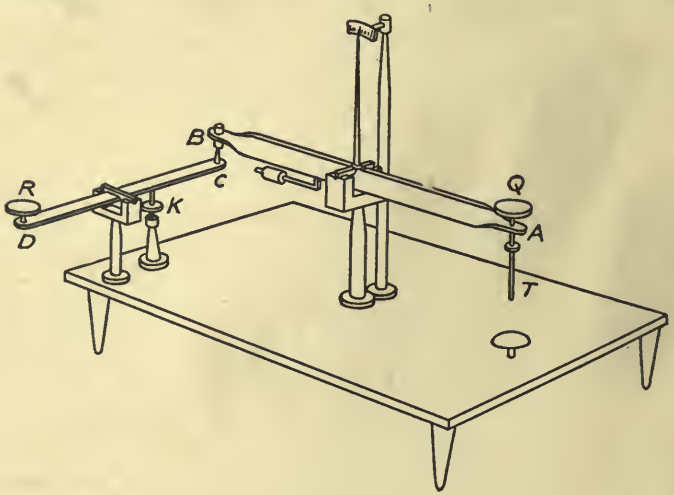


FIG. 1.

badly in that the subject's hand often shifts slightly or changes in volume, when the lower end of *T* may easily come to lose contact with the cork, or on the other hand, may come to press tightly on it. Hence *K* was not used at all, but instead a 3-gr. weight was allowed always to remain on the pan *R*, and this kept *T* in continual light contact with the cork in a very satisfactory manner. The weights were applied to the pan *R* by putting them on a piece of stiff but soft blotting paper and lowering them gently to the pan, a thing which required much practice on the part of the operator.

§3. *Procedure.*—The operator gave a signal by saying 'One!' in a low voice and then placed the weight on the pan *R*, removing it after four seconds. After an interval also of four seconds, he gave the signal 'Nine!' and placed the second

weight on the pan, removing it again after four seconds. The subject pronounced judgment with respect to the second weight saying that it was heavier, lighter or the same as the first.

The session with each subject occurred once per week, the actual work lasting usually about forty or forty-five minutes. The first part of the period usually was devoted to practice, but the number of judgments recorded in a successful session averaged between forty and eighty. During the session rest-periods of a few minutes each were given,—sometimes one or three, but more often two. Nevertheless there were times when the subject complained of cramp of the arm, which came from keeping it in a constant position. While fatigue of the sense-organ had to be taken into account and as much as possible avoided, yet it seemed to present no grave difficulty. The position of the cork disc on the hand was changed now and then at the instance either of the operator or of the subject, the latter being requested to move the disc when it seemed necessary.[1] When the attention was good, the judgments proved on the whole as reliable toward the end of the hour as at the beginning. We may add the detail that there was daylight illumination, but that the subject sat with closed eyes. Unfortunately the room was not sound-proof, and hence it was frequently necessary to wait until some outside noise should cease, and even to repeat a set of weights which had been judged. A repetition was also made whenever in the judgment of the subject or operator the weights were put on the pan in improper manner. Repetitions were specially recorded in the protocol except in the first part of Series I. A special sign was used to mark whenever the subject found that owing to poor attention or the like he was in doubt and unable to judge. Every effect was made to have him properly distinguish between cases in which he ought to judge 'the same' and those in which he ought to report himself really unable to judge. In all likelihood, however, many cases were pronounced 'doubtful' which should have been called 'the same' and *vice versa*, for the distinction is often very difficult

[1]Rests and changes of the position of the cork were recorded in the protocol.

indeed. It scarcely need be added that the operator continually had in mind the danger of unconsciously communicating to the subject anything that might influence his judgments. Hence care was taken in uttering the signals and in other similar respects. The personal equation of the operator in this series certainly introduces a large error, but in a subsequent series where sounds mechanically produced were judged instead of weights, the general character of the results obtained were the same as those of this series, and hence that error may to some extent be disregarded. So far as possible, the ultimate aims of the experiment were kept from the knowledge of the subject.

It will be recalled that the method of procedure is to select a convenient weight *S* which produces a pressure sensation of moderate strength and then to find two other weights *C* and *C′*, one lighter and the other heavier, as described on page 26. Because of the well-known tendency to overestimate the strength of the second of two sensations, it is necessary to distinguish between the orders *S–C* and *C–S*. For if *S* is judged heavier than *C* a certain number of times out of ten when given in the former order, one may expect it to be judged heavier a different number of times out of ten in the latter order.[1] We have, then, the couplets *S–C*, *C–S*, *S–C′* and *C′–S*, which we shall designate respectively by the letters A, B, E and F. Our purpose is to find the number of times out of ten that *S* is judged heavier than *C* or *C′*, in each of these orders, and then to produce the sound simultaneously with *C* and *C′* (but not with *S*) and to find how many times *S* is then judged heavier. Accordingly, if we let the symbol */n* stand for the occurrence of the sound, we have also the couplets *S–C/n*, *C/n–S*, *S–C′/n*, and *C′/n–S*, which we shall designate respectively by the letters C, D, G and H. Because of personal peculiarities of sensibilitity and discrimination power, different sets of weights were used for each subject, and this is indicated in Table I. For example, it will be seen that for subject 1 *S* = 14, *C* = 10, and *C′* = 18, while for subject 2 *S* = 20, *C* = 15, and *C′* = 25.

[1]Usually a larger number of times in the order *C–S*, according to my own experience.

TABLE I.[1]

	Subject 1.		Subject 2.		Subject 3.		Subject 4.		Subject 5.	
	First Wt. Given (gm.).	Second Wt. Given (gm.).	First Wt. Given (gm.).	Second Wt. Given (gm.).	First Wt. Given (gm.).	Second Wt. Given (gm.).	First Wt. Given (gm.).	Second Wt. Given (gm.).	First Wt. Given (gm.).	Second Wt. Given (gm.).
A	14	10	20	15	22	18	24	18	20	10
B	10	14	15	20	18	22	18	24	10	20
C	14	10/*n*	20	15/*n*	22	18/*n*	24	18/*n*	20	10/*n*
D	10/*n*	14	15/*n*	20	18/*n*	22	18/*n*	24	10/*n*	20
E	14	18	20	25	22	26	24	30	20	30
F	18	14	25	20	26	22	30	24	30	20
G	14	18/*n*	20	25/*n*	22	26/*n*	24	30/*n*	20	30/*n*
H	18/*n*	14	25/*n*	20	26/*n*	22	30/*n*	24	30/*n*	20

Since the subject is instructed to pass judgment in terms of the second weight given it will be apparent that if we count the number of judgments 'lighter' in the cases of A, C, E and G, but the number of judgments 'heavier' in the cases of B, D, F and H, we shall in each case have a figure representing the number of times that *S* is judged heavier than its mate. It is obvious also that A is to be compared with C, B with D, E with G, and F with H; or similarly, (A+B) with (C+D), and (E+F) with (G+H); or again, (A+B+E+F) with (C+D+G+H). Following a practice that is general in experiments with weight sensations, we reckon the judgment 'same' as equivalent to one half 'heavier' or one half 'lighter.' In other words 'same' means midway between 'heavier' and 'lighter.' Series 1 consisted of four parts in each of which ten judgments were passed on each of the couplets A, B, C, D, E, F, G, H given in haphazard order. Hence altogether there were 4×10 judgments passed on each of eight couplets, making a total of 320 judgments for each subject.

§4. *Results.*—The results of Series 1 are presented in Table II. As the explanatory note under the table indicates, (which see) each figure in roman type is to be compared with the corresponding one in italic. Even a careless glance will show that the italic figures are in a good majority of the cases larger than those in roman. Out of the eighty cases in this table where one may compare the figure for a single letter in roman (A or B or E or F) with the corresponding one in italic, an increase occurs

[1]The sign (/*n*) signifies the occurrence of the sound.

53 times, an equality 14 times, and a decrease only 13 times. The increase is made especially clear in the sums in the table. It can also be shown in the averages;—for instance, the total averages for all subjects reckoned together are as follows: A=7.5, B=8.2, C=8., D=8.7, E=1.7, F=3.5, G=2.8, H=4.6.

TABLE II.

		Subject 1.		Subject 2.		Subject 3.		Subject 4.		Subject 5.	
Series 1–A.											
A	*C*	9	*10*	4½	*8*	8½	*8*	7	*7*	9	*9*
B	*D*	9	*9*	9	*9*	8	*8*	7	*7½*	9	*10*
A+B	*C+D*	18	*19*	13½	*17*	16½	*16*	14	*14½*	18	*19*
E	*G*	3	*5*	0	*1½*	2	*5*	6	*3½*	0	*2½*
F	*H*	3	*4*	5½	*4½*	6	*8*	1½	*3*	3	*4½*
E+F	*G+H*	6	*9*	5½	*6*	8	*13*	7½	*6½*	3	*7*
A+B+E+F	*C+D+G+H*	24	*28*	19	*23*	24½	*29*	21½	*21*	21	*26*
Series 1–B.											
A	*C*	8	*9*	3½	*5*	5	*7*	8	*8*	9½	*8*
B	*D*	8	*8½*	10	*10*	9	*6*	7½	*10*	10	*9½*
A+B	*C+D*	16	*17½*	13½	*15*	14	*13*	15½	*18*	19½	*17½*
E	*G*	1	*2*	1	*6½*	4	*2*	½	*2*	2	*1*
F	*H*	4½	*5*	5	*7½*	4	*9*	4	*4*	2	*2*
E+F	*G+H*	5½	*7*	6	*14*	8	*11*	4½	*6*	4	*3*
A+B+E+F	*C+D+G+H*	21½	*24½*	19½	*29*	22	*24*	20	*24*	23½	*20½*
Series 1–C.											
A	*C*	9½	*7½*	8	*9½*	7	*8*	6½	*8*	7½	*9½*
B	*D*	7½	*9*	9	*7*	8	*9*	10	*10*	10	*10*
A+B	*C+D*	17	*16½*	17	*16½*	15	*17*	16½	*18*	17½	*19½*
E	*G*	1	*5*	4	*4*	2½	*4*	0	½	½	*1½*
F	*H*	2½	*3*	4½	*5½*	3	*4*	4	*2*	2½	*5½*
E+F	*G+H*	3½	*8*	8½	*9½*	5½	*8*	4	*2½*	3	*7*
A+B+E+F	*C+D+G+H*	20½	*24½*	25½	*26*	20½	*25*	20½	*20½*	20½	*26½*
Series 1–D.											
A	*C*	8½	*9*	8	*8½*	7	*5*	7	*8½*	8	*8*
B	*D*	5	*8*	8	*5½*	4	*9*	8	*9*	8½	*10*
A+B	*C+D*	13½	*17*	16	*14*	11	*14*	15	*17½*	16½	*18*
E	*G*	4	*2½*	0	*3½*	1	*2*	½	*1½*	1	*1*
F	*H*	2½	*4*	2	*2*	4	*5*	3	*5*	3½	*4½*
E+F	*G+H*	6½	*6½*	2	*5½*	5	*7*	3½	*6½*	4½	*5½*
A+B+E+F	*C+D+G+H*	20	*23½*	18	*19½*	16	*21*	18½	*24*	21	*23½*
Total sum	*Total sum*	86	*100½*	82	*97½*	83	*99*	80½	*89½*	86	*96½*

Figures opposite single letters in roman type (A or B or E or F) in each case represent the number of times a given weight (*S*) was judged heavier than another (*C* or *C'*); while in the space to the right of each such figure in roman is a corresponding figure in italic which represents the number of times that same weight (*S*) was judged heavier than the same other one (*C* or *C'*) when the sound was made simultaneously with the latter.

In other words, the number of times that a given weight, S, was judged heavier than another was on the whole increased when a sound was made simultaneously with the application of that other. That the influence of the sound was not essentially to confuse the judgment was learned firstly from the reports of the subjects who soon became accustomed to it, and secondly, it is shown by the figures themselves. For it is generally true, as we have seen, that not alone are the figures for E and F which average respectively 1.7 and 3.5 materially increased in G and H, but what is especially significant, so also are the figures for A and B increased in C and D, despite the fact that these average above 7. Hence the figures seem conclusively to indicate that the sound diminishes the weight sensation.[1] The subjects thought themselves to some extent able to verify the inhibiting effects of the sound when (after the year's work was over) they were told of it and special tests were repeated. Previously, however, they had not been able to guess the effect of the sound for themselves and similarly they were not able to notice that when sounds occurred in the hall or out-doors their judgments were affected thereby. *This indicates the interesting fact that one may be distracted and yet be unaware of it.*

That the diminution is not due to the memory but lies rather in the sensation process itself is shown by a simple calculation: In the cases of D and H, the sound occurs with the first weight which is remembered and thus compared

[1]Upon being questioned as to whether the sound made the concomitant pressure less 'clear,' three of the subjects replied that it did in some cases, but often or usually it did not seem to. In this connection it must be remembered that the sound was repeated again and again in the course of the experiment. Two of the subjects thought it possible or likely that the sound had a characteristic effect to reduce the clearness of the pressure sensation. The subjects were not in agreement in their opinions as to whether a reduction of clearness in the pressure sensation would lead them to judge the corresponding weight lighter than otherwise, some believing it might, others that it would not. However, if confidence in judgment is a function of the clearness of sensations judged, then it is pertinent to mention that for the most part, though not always, the subjects said that they had about the same degree of confidence in the truth of their judgements when the sound came as when it did not. Excepting, however, that the operator frequently noted not a decrease but rather a very marked increase in confidence as expressed in the tone of voice with which a given weight was judged heavier than a lighter one when the sound occurred with the latter.

with the second. But the total average increase from B to D is—as calculated from the figures just given—.5 while that from F to H is 1.1. Now in the cases of C and G the sound occurs with the second weight and here the subject often judges while the latter is still on his hand and hence memory in no way enters in as a significant factor. But the increases from A to C and from E to G are respectively .5 and 1.1, thus showing that the diminution of the sensation is not due to fading of the memory image.

The subjects remarked that they often judged heaviness by the distance which the rod *T* seemed to push down into the flesh. To the extent to which they did this they were estimating weights perhaps not simply by strengths of sensation but rather by qualitative characteristics of the sensations. But in the latter event one would reasonably expect that the sound would have no influence on their judgments of heaviness, excepting, perhaps, to render them less confident and constant. If, therefore, one finds—as we actually have—that it is in general true that a sound concomitant with a pressure tends to make the corresponding weight judged less heavy than otherwise, it seems warranted to conclude that *the sound diminishes the strength or intensity of the weight sensation.*[1] To be sure additional evidence is very welcome, and this is furnished by analogy in Series 2.

Series 2.

Inhibition of Sound Sensations by Pressure Sensations.

§5. *Apparatus.*—In this series it was undertaken to discover whether sound sensations of moderate strength can be inhibited by pressure sensations. The gravity phonometer is generally used to make sound sensations of various intensities, but it is very inferior to the tuning-fork which may

[1]The strength of the conscious sensation—the subjective intensity—is meant here since the sound of course does not interfere with the objective stimulus. It is convenient to give the name of objective intensities to correspond proportionately with the objective stimuli, though these are essentially fictional, and not real entities, since the real intensity of a conscious sensation—as we seek here to demonstrate—depends not solely on the quantity of the objective stimulus, but decidedly upon subjective conditions as well.

be arranged as shown in the accompanying sketch (Fig. 2). An electrical tuning-fork of 256 vibrations was placed in an adjoining room and connected in single series with a key *C*, a rheostat *D* and a telephone receiver *G*, to which was attached a megaphone *H*. To the rheostat ('*G–R*' rheostat, with Gebr. Ruhstrat composition-wire resistance) was attached a millimeter scale along which the slider *E* moved, thus getting variation of the current that passed through the tele-

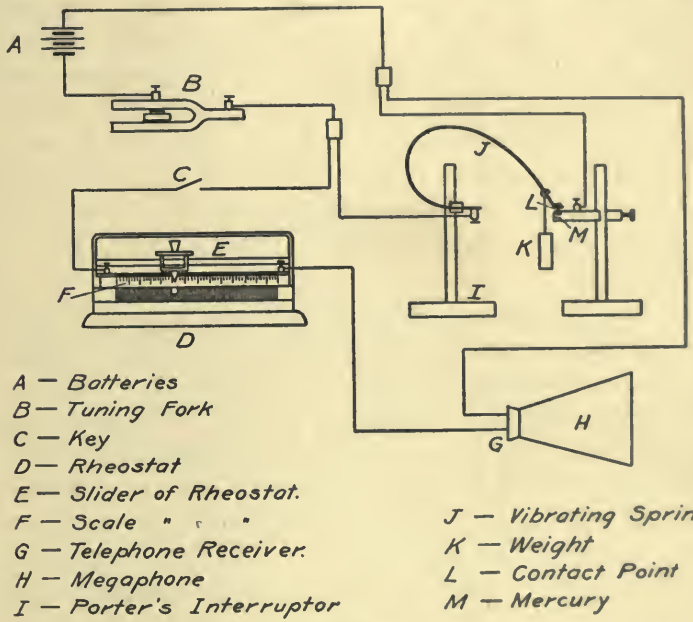


FIG. 2.

phone receiver and hence getting corresponding variation of the loudness of the sound.

If the apparatus consisted merely of the fork, rheostat, megaphone, key and connecting wires, one could not produce sounds of short duration at will, for it would be necessary to go into the other room to start up the tuning-fork each time a sound were desired. Now it is necessary so to arrange the apparatus that the tuning-fork is kept going all the time. To this end the leads from the tuning-fork were also connected up with wires leading through the apparatus marked *I*.

This instrument is known as 'Porter's Interrupter' and consists of a spring *J* which is bent over and held down by a weight *K*. The spring can be arranged with various vibrating lengths, but in the present experiment it was so arranged that it made a vibration in half a second. The contact point *L* was arranged so that it just touched the surface of the mercury *M*. Now when *K* was pulled down a short distance by the hand of the operator and then released, *L* would rise out of the mercury and thus break the circuit for one half second. With the key *C* closed and *L* in contact with *M* practically all the current would always pass through the wires that lead through *J*, because here the resistance was very much less than through *D*. But upon breaking contact with *M*, the current would be forced to go through *D* and *G*, thus making a sound one half second in duration, the relative intensity of which could be fixed at will by moving the slider *E*.[1]

For the pressure sensations the apparatus was used as shown in Fig. 3. The hand of the subject was placed as before with the cork disc upon it under *T*. A constant weight of 10 gr. was placed under the pan *R*, thus keeping *T* in contact with the cork. Throughout Series 2, the pans *R* and *S* each bore a weight of 300 gr. The one, however, causes pressure down on the point *B* and the other up, so that each neutralizes the other so far as any motion of the lever *A–B* is concerned. In order to apply a sudden pressure of 300 gr. to the cork on the subject's hand, the operator suddenly pressed down on the lever *E–F* at the point *F*. Since the pressure sensation should be simultaneous with the sound it was very important for the operator to press down on *F* with the left hand simultaneously with the commencement of the sound. The pressure on the hand of course outlasted the sound, for it was not possible under the conditions to make its duration only one half second. The essential thing, however, is to have the pressure commence with the sound and last at least as long.

[1]Of course what is actually secured is a satisfactory approximation to one half second rather than the precise attainment. It makes for accuracy to pull down *K* an equal distance each time.

§6. *Procedure.*—On the whole this was like that of Series 1, except that the signals 'One!' and 'Nine!' were omitted, the only signal used being the word 'ready' followed by the sound a little thereafter. The second sound occurred six seconds after the first. The subject was asked to judge intensities (*i. e.*, loudness) of sound and to reply 'fainter' or 'louder,' or 'same' with reference to the second sound. Slight qualitative differences in the sound did, it is true, enter in as an error, and the subject was warned against confusing these with quantitative differences and against associating qualitative peculiarities with any given intensity, so as to try to identify the latter thereby. For while a somewhat

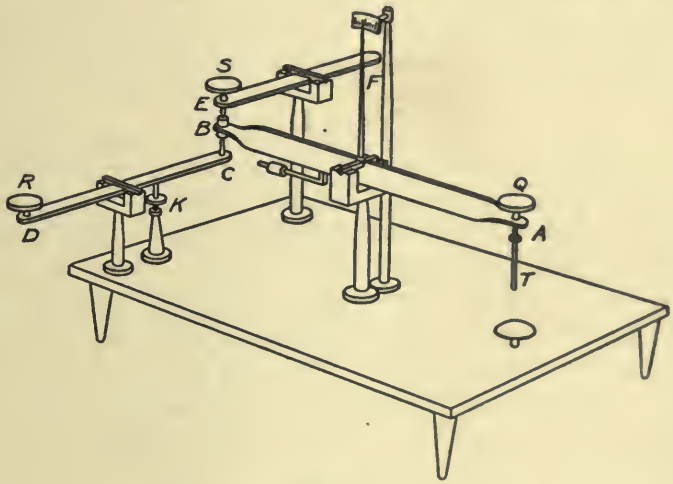


FIG. 3.

raucous tone, for instance, often characterized the stronger sounds, so also did it sometimes characterize the faint sounds, and hence it was explained to the subjects that they must not be misled thereby. Therefore, while the judgments doubtless were frequently influenced by qualitative differences, we are safe in believing that what was compared on the whole was essentially intensities of sounds.

At first the operator was in doubt as to what duration the sound should have. Various durations such as 3, 2 and 1 second were tried during the period when he was trying to

learn what was the requisite technique. Usually the practice results with these durations showed an inhibiting effect of the pressure on the sound, but often they did not. In order to obtain exact results the operator concluded that the proper duration of the sound should be one half second, firstly because this would largely preclude the possibility that the subject's attention should adapt itself to the pressure while the sound was still in operation, and secondly because it is possible that the pressure sensation quickly loses in strength.

After the subjects had been given considerable practice the operator determined what strengths of sound should be given to each of them. Because of personal differences in discrimination power, the same differences of sound could not be used for all subjects alike. Table III. indicates the relative strengths of sounds given together with the order in which

TABLE III.

	Subject 3.		Subject 4.		Subject 9.		Subject 5.	
	First Sound Given.	Second Sound Given.	First Sound Given.	Second Sound Given.	First Sound Given.	Second Sound Given.	First Sound Given.	Second Sound Given.
A	6.	7.	6.5	8.	6.2	7.4	8.7	9.4
B	7.	6.	8.	6.5	7.4	6.2	9.4	8.7
C	6.	7./300	6.5	8./300	6.2	7.4/300	8.7	9.4/300
D	7./300	6.	8./300	6.5	7.4/300	6.2	9.4/300	8.7
E	6.	5.	6.5	5.	6.2	5.	8.7	8.
F	5.	6.	5.	6.5	5.	6.2	8.	8.7
G	6.	5./300	6.5	5./300	6.2	5./300	8.7	8./300
H	5./300	6.	5./300	6.5	5./300	6.2	8./300	8.7

they occurred. The numbers stand for sounds varying with the positions of the slider *E* of the rhesotat and have no precise significance except that the smaller the number, the louder is the corresponding sound. The sign /300 indicates the application of a pressure of 300 gr. We shall once more use the symbol *S* for the standard sound (6. for Subj. 3), while *C* represents the fainter, and *C'* the louder sound compared with it.

§7. *Results.*—It will be seen that the results shown in Table IV. are of analogous nature to those of Series 1. The italic figures are larger than the corresponding roman ones in 29 out of 32 cases, and this means that a given sound (*S*) was

judged louder than another sound (*C* or *C'*) when a simultaneous pressure occurred with the latter than when it did not. We are permitted to infer that *the conscious intensity of sound sensations may be reduced by concomitant pressure sensations*, a thing which harmonizes with and helps to confirm the results of Series 1.

These results are in agreement, moreover, with those of an experiment performed by Bentley, who used various sounds produced by means of the gravity phonometer with odors as distractives. He found that the odor caused the sound with which it was given to be 'underestimated,' and concluded that distraction may reduce the intensity of sensation.[1] Heymans, also, asserts that sensations above the limen may suffer a partial loss of intensity, saying that in his experiments when a given stimulus was able to produce a faint sensation despite the presence of another stronger sensation, then upon the removal of the latter the subject reported a perceptible increase of intensity. Other evidence, he claims, lies in the well known phenomena of intensity-contrast, and he undertakes special experiments in light and brightness contrast to show that this is so.[2] Finally there are the experiments of McDougall which show inhibition in the rôle of a reduction of psychic intensity. On page 185 of his 'Inhibitory Processes within the Nervous System' (*Brain*, 26, 1903) he speaks of 'sensation-intensity' as being 'subjectively diminished.' Again in a discussion of Fechner's paradox in connection with inhibition he speaks of 'loss of brightness,' which is of course loss of visual intensity.[3] And when in his series of articles on vision in *Mind*, 26, N. S., 10, he continually speaks of the 'fading' of images he is apparently speaking of loss of intensity. Hence there seems in general no room for doubt but that the subjective intensity of sensations may be reduced by the presence of other sensations.

[1]Bentley, PSYCHOL. BULLETIN, IV., 1907, 212 f. Referred to by Titchener *Psychol. of Feeling and Attention.*

[2]Heymans, *ibid.*, 41, 1906, 28 f. and 89 f.

[3]*Brit. Jour. of Psy.*, 1, 1904–05, 114–115.

TABLE IV.

		Subject 3.		Subject 4.		Subject 9.		Subject 5.	
Series 2–A.									
A	*C*	5½	*8½*	4½	*6½*	4	*10*	6½	*7½*
B	*D*	8	*10*	9½	*9½*	9	*10*	7½	*9½*
A+B	*C+D*	13½	*18½*	14	*16*	13	*20*	14	*17*
E	*G*	½	*1*	1	*2*	0	*5*	0	*4*
F	*H*	4½	*7*	4	*7½*	5	*9*	5	*6*
E+F	*G+H*	5	*8*	5	*9½*	5	*14*	5	*10*
A+B+E+F	*C+D+G+H*	18½	*26½*	19	*25½*	18	*34*	19	*27*
Series 2–B.									
A	*C*	7	*8*	0	½	2	*8*	5½	*9½*
B	*D*	9	*10*	2½	*7½*	8	*10*	7	*9*
A+B	*C+D*	16	*18*	2½	*8*	10	*18*	12½	*18½*
E	*G*	½	*0*	7	*9½*	1	*3*	3	*6*
F	*H*	4	*4*	9½	*10*	8	*9*	2½	*7½*
E+F	*G+H*	4½	*4*	16½	*19½*	9	*12*	5½	*13½*
A+B+E+F	*C+D+G+H*	20½	*22*	19	*27½*	19	*30*	18	*32*
Total sum	*Total sum*	39	*48½*	38	*53*	37	*64*	37	*59*

Figures opposite single letters in roman type (A or B or E or F) in each case represent the number of times a given sound (*S*) was judged louder than another one (*C* or *C'*); while in the space to the right of each such figure in roman is a corresponding figure in italic which indicates the number of times *S* was judged louder than *C* or *C'* when a pressure of 300 gr. was simultaneously applied to the right hand.

SERIES 3.

The Inhibitory Effect on Sound Sensations of Pressure Sensations of Different Intensities.

§8. *Apparatus.*—For sounds, same as in Series 2. For pressure sensations likewise, except that here weghts of 100 gr., 300 gr., and 500 gr. were used.

§9. *Procedure and Results.*—Only two subjects were used in this series (subjects 5 and 2). The subjoined table (V.) is practically self-explanatory. It shows, for instance, in the column headed 'Series 3–A' that the sound 5.7 was judged louder than 5 one time out of ten when there was no simultaneous pressure, one time out of ten when a pressure of 100 gr. occurred with 5, also one time with a pressure of 300 gr., and four times with a pressure of 500 gr. Thus the reader will be able easily to interpret the table. The observations are of course too few in number to permit quantitative formulations, but at least they suggest it as probable that *the greater*

the stimulus the stronger tends to be the inhibitory power of the corresponding sensation, a thing which one would naturaly presume to be true.

Series 4.

The Inhibitory Effect of Inattention on Pressure Sensations.

§10. *Introduction.*—Hereupon a very interesting problem develops itself. It is apparently a very fundamental relation of mind if when two sensations are simultaneously presented, the one may inhibit the other. Now what part, if any, does attention play in all this? May we assume, as would be quite natural, that what the inhibitor does is substantially to dis-

Table V.[1]

Subject.		First Sound Given.	Second Sound Given.	Number of Times Out of Ten First Sound Judged Louder. Series 3-A.	Series 3-B.	Series 3-C.	Series 3-D.	Total.
5	A	5·7	5·	1	1	2½		4½
	B	5·7	5·/100	1	3	2		6
	C	5·7	5·/300	1	1	1		3
	D	5·7	5·/500	4	3½	2½		10
2	A	6.	5·	1½	1½	0	1	4
	B	6.	5·/100	1½	1	1	2	5½
	C	6.	5·/300	3½	2	1	½	7
	D	6.	5·/500	6	2	2	2	12
		First Sound Given.	Second Sound Given.	Number of Times Out of Ten Second Sound Judged Louder. Series 3-A.	Series 3-B.	Series 3-C.	Series 3-D.	Total.
5	E	5·5	6.	3½	5	5		13½
	F	5·5/100	6.	8	7½	3		18½
	G	5·5/300	6.	5	7	6		18
	H	5·5/500	6.	8	8½	5½		22
2	E	5·3	6.	2½	5	5	4½	17
	F	5·3/100	6.	7½	6	4½	3½	21½
	G	5·3/300	6.	8	6½	7½	3	25
	H	5·3/500	6.	8	6½	9	8½	32

tract the attention from the inhibited? It seems inviting not to asume this, but rather to institute a new inquiry whether it is so. Thus we have encountered the old and difficult question whether increased attention brings increased intensity to sensation. But in certain respects we have unusual facili-

[1]Refer to p. 40.

ties for its solution, in as much as we are already supplied by the foregoing series with very efficient distractives together with the correct means to use them. The way is, namely, so to arrange the conditions that the distractive begins with the sensation distracted from. The importance of this—which I believe is not generally understood—deserves special emphasis. For if the the inception of the distractive is before or long after that of the sensation to be distracted from, it is likely to lose much of its inhibitory power, and it is even possible—so far as we at present know—that it may under certain conditions augment instead of inhibit. It is imperative, moreover, that the sensation distracted from be not of too long duration. Else once more the distractive—by virtue of a principle which we shall later develop—may lose its initial inhibitory influence and eventually indirectly produce an augmentation. Doubtless here lies the source of the conflicting reports well known to the literature about distractives.

Hence we need to make the time-periods short as well as simultaneous. One more special condition needs to be fulfilled if we are to develop a precise means of treatment of the problem of voluntary attention and intensity. This condition requires that contrary to the usual methods, the distractive must not be absent in one set of observations and present in the other set which is to be compared therewith, but is rather to be present in both sets, the important point of difference being that in one set the attention is voluntarily put on the distractive, while in the other it is not. Thus we will be permitted to abstract from the influence of the distractive as such, and may learn what are the differences produced by the voluntary attention alone. This passage will become clearer after we have presented and explained our tables.

§11. *Apparatus and Procedure.*—The apparatus was the same as that of Series 1, pressure sensations of four seconds in duration being produced and judged. The principle of procedure was to select a convenient weight *S*, together with a series of five other weights and to make two classes of observations with the purpose of comparing their results: first, to find out how many times out of ten the *S* weight—

always given first—would be judged heavier than each of these weights if the sound always occurred with the application of the latter while the attention was allowed naturally to fall on the pressures which were to be compared; and second, to find out how many times *S* would be judged heavier than each of these weights under the same conditions as before except that the attention were kept on the sound during its occurrence, but after its cessation transferred to the right hand, so that the sensation from the *S* weight was always received with normal or undistracted attention. In Table VI., which shows what weights were used, the capital letters A, B, C, D, E, serve to designate the couplets of weights compared with the attention resting naturally on the weight

TABLE VI.

	Subject 6.		Subject 7.		Subject 8.		Subject 9.	
	First Wt. Given (gm.).	Second Wt. Given (gm.).	First Wt. Given (gm.).	Second Wt. Given (gm.).	First Wt. Given (gm.).	Second Wt. Given (gm.).	First Wt. Given (gm.).	Second Wt. Given (gm.).
A	18/*n*	20	12/*n*	15	10/*n*	12	10/*n*	12
B	20/*n*	20	15/*n*	15	12/*n*	12	13/*n*	12
C	23/*n*	20	18/*n*	15	13/*n*	12	16/*n*	12
D	25/*n*	20	21/*n*	15	15/*n*	12	19/*n*	12
E	28/*n*	20	24/*n*	15	18/*n*	12	22/*n*	12
a	18/*n*	20	12/*n*	15	10/*n*	12	10/*n*	12
b	20/*n*	20	15/*n*	15	12/*n*	12	13/*n*	12
c	23/*n*	20	18/*n*	15	13/*n*	12	16/*n*	12
d	25/*n*	20	21/*n*	15	15/*n*	12	19/*n*	12
e	28/*n*	20	24/*n*	15	18/*n*	12	22/*n*	12

sensations, while the small letters, a, b, c, d, e, designate the couplets in which the attention was voluntarily concentrated on the sound. The sign /*n* stands for the sound.

Of course practice was necessary before the subjects learned to concentrate on the sound (*i. e.*, in the cases of the small letters). They were told that in case such concentration made them unable to judge with confidence, they should mention the fact and withhold their reports if necessary. Their experience was, however, that when the concentration was directed to the sound and the concomitant pressure therefore less strongly attended, there nevertheless remained a good memory image of that pressure which enabled them to judge it in a very satisfactory way.

Subjects 6 and 7 seemed to acquire the needed control of the attention with great readiness. Subject 8 usually reported the control good, but the operator frequently had doubts about the matter.[1] Subject 9 admitted that he was easily distractible by noises and the like occurring while he was studying, and he sometimes stated that his attention to the sound was not good. It was necessary to give emphasized instructions not merely to attend to the sound during the first pressure application but in all cases to return to the second pressure with normal attention, else there was danger that the second weight would be insufficiently attended. In order to stimulate the subjects to proper effort the operator reminded them very frequently (sometimes each time) what was to be done—if only with a word.

In taking the protocol ten capital letters were first given and then ten small letters or *vice versa*, care being taken to equalize conditions. That is to say, ten judgments were passed in succession with the attention voluntarily directed to the sound in each case, and next ten with the attention not on the sound but naturally directed toward the pressures on the right hand which were to be discriminated. Judgment was always passed in terms of the second pressure, and the numbers in Table VII. represent the number of times the second weight was judged heavier.

§12. *Results.*—It will be noticed that the results are very pronounced indeed. The italic numbers show a great increase over the roman. That is to say, the number of times a given weight is judged heavier than another is much increased if the attention is not directed toward the latter. *Decrease of attention to a pressure seems therefore to involve decrease of intensity of sensation.*[2]

[1]At such times on several occasions he stopped taking the records in order to give the subject special practice, a thing also done with subject 9.

[2]Subject 3 also was tried for this series. This subject, however, found the rattling sound of the buzzer very disagreeable and apparently was not able to perform the desired concentration. Hence only 3–A was performed with this subject. It is interesting to state that here A+B+C+D+E=30 while a+b+c+d+e=23½, showing a decrease where with proper attention there presumably would have been an increase. The instruction to concentrate on the sound, instead of being carried out, led to the opposite achievement.

Therefore we are warranted in inferring that in Series 1 and 2 the diminution of the one sensation by another was identical with a distraction of the attention. Or rather, it is more illuminating to put it the other way: *in the case of simultaneous sensations, distraction of the attention is equivalent to diminution or inhibition of one by the other.* Also, some of the subjects reported that attention away from the pressure seemed to give its sensation a changed quality. We may perhaps explain this by remembering the principle elaborated by Heymans that faint sensations may be utterly suppressed

TABLE VII.

		Subject 6.		Subject 7.		Subject 8.		Subject 9.	
Series 3–A.									
A	*a*	9	*9*	8	8½	6	*7*	4	*7*
B	*b*	5	*7*	5½	*6*	4½	*4½*	6	*3*
C	*c*	3	*10*	6½	*10*	4	*2*	4	*7*
D	*d*	4	*8*	6	*8*	7	*4*	4	*4*
E	*e*	3	*5*	5	*7½*	4	*3½*	2	*4*
Sum	*Sum*	24	*39*	31	*40*	25½	*21*	20	*25*
Series 3–B.									
A	*a*	7	*10*	8½	*7*	8	*7*	6	*4*
B	*b*	9	*8*	3	*7*	5	*8*	3	*5*
C	*c*	8	*9*	5½	*9*	4	*9*	2	*3*
D	*d*	5	*8*	5	*8*	3	*6*	1	*1*
E	*e*	5	*2*	2	*7*	2	*2*	3	*3*
Sum	*Sum*	34	*37*	24	*38*	22	*32*	15	*16*
Series 3–C.									
A	*a*	7	*8*	9	*9*	9	*8*	7	*7*
B	*b*	5	*9*	6½	*7½*	4	*6*	5	*7*
C	*c*	8	*8*	5½	*8½*	5½	*9*	2	*3*
D	*d*	7	*7*	4	*5½*	5	*6*	4	*3*
E	*e*	3	*5*	1	*5*	3½	*4½*	0	*2*
Sum	*Sum*	30	*37*	26	*35½*	27	*33½*	18	*22*
Series 3–D.									
A	*a*	7	*7*	7	6½	9½	*6*	7	*6*
B	*b*	9	*10*	5½	*9*	4	*5*	3	*5*
C	*c*	5	*7*	4	*7*	6	*6*	5	*7*
D	*d*	5	*6*	5½	*8½*	5½	*6*	3	*2*
E	*e*	2	*7*	4½	*7*	3	*6*	2	*2*
Sum	*Sum*	28	*37*	26½	*38*	28	*29*	20	*22*
Total sum	*Total sum*	116	*150*	107½	*151½*	102½	*115½*	73	*85*

Each figure in roman type opposite A or B or C or D or E represents the number of times out of ten that a certain weight *S* was judged heavier than some other one under conditions described in the context. In the space to the right of each such figure in roman is one in italic which represents the number of times out of ten that *S* was judged heavier than that same other weight under the same conditions as before except that the latter weight received less attention.

by other strong ones. Now our pressure sensation was not a simple sensation, but rather a group of sensations—some of different qualities and strengths—some of light surface contact, others of deeper pressure. Therefore when one subject reports that withdrawing the attention from the pressure makes the sensation become 'dull and heavy,' we may reasonably suppose that this is due to the fact that the fainter sensations become suppressed, leaving the stronger ones as the residue and thus changing the character of the whole. It is interesting to think that increasing the attention of such a complex sensation is like increasing the volume of a clang, for thereby one changes not alone its quantity but its quality by making the 'overtone' sensations, so to call them, become more and more perceptible.

Series 5.

The Inhibitory Effect of Inattention on Sound Sensations.

§13. *Apparatus and Procedure.*—This is in principle the same as 3, but instead of judgments about pressure sensations with sounds as distractives, the judgments were made concerning sounds with pressures for distractives. The same instruments for pressures and sounds were used as those of Series 2, and the sounds here were also one half second in duration. Of course the operator made every effort to observe the caution that the pressure be applied simultaneously with the beginning of the sound. Table VIII. shows the relative strengths of sounds used, and the sign /300 indicates that a pressure of 300 gr. was applied with the first sound.

Table VIII.

	Subject 6.		Subject 7.		Subject 8.	
	First Sound Given.	Second Sound Given.	First Sound Given.	Second Sound Given.	First Sound Given.	Second Sound Given.
A	5. /300	8.	5./300	7.	6./300	8.5
B	6. /300	8.	6./300	7.	7./300	8.5
C	6.7/300	8.	7./300	7.	8./300	8.5
D	8. /300	8.	8./300	7.	9./300	8.5
a	5. /300	8.	5./300	7.	6./300	8.5
b	6. /300	8.	6./300	7.	7./300	8.5
c	6.7/300	8.	7./300	7.	8./300	8.5
d	8. /300	8.	8./300	7.	9./300	8.5

The numbers stand for strengths of sounds: the smaller the number the louder the sound. The sign (/300) stands for the application of the 300 gr. pressure. In the couplets of sounds designated by the capital letters the attention rests naturally on the sounds to be discriminated. In those designated by the small letters the first sound receives less attention than in the case of the capital letters for it is voluntarily placed on the pressure sensation; but the second sound receives normal attention.

§14. *Results.*—The numbers in Table IX. represent the number of times out of ten that the second sound was judged louder. The results are similar to and in harmony with those of Series 4. The italic figures are in 19 out of 24 cases

TABLE IX.

		Subject 6.		Subject 7.		Subject 8.	
Series 5–A.							
A	*a*	0	*4*	1	*4*	1½	*5*
B	*b*	1	*4½*	2½	*8*	5	*6*
C	*c*	5	*6½*	4½	*9*	8	*9*
D	*d*	7½	*6½*	10	*9*	8	*9½*
Sum	*Sum*	13½	*21½*	18	*30*	22½	*29½*
Series 5–B.							
A	*a*	2	*2½*	2	*3½*	1½	*3½*
B	*b*	2½	*6½*	1	*8*	7	*7*
C	*c*	7½	*7*	7½	*10*	8	*7*
D	*d*	6½	*9*	9	*10*	9	*9½*
Sum	*Sum*	18½	*25*	19½	*31½*	25½	*27*
Total sum	*Total sum*	32	*46½*	37½	*61½*	48	*56½*

Each figure in roman type opposite A or B or C or D represents the number of times out of ten that a certain sound *S* was judged louder than some other one under conditions described in the context. In the space to the right of each such figure in roman is one in italic which represents the number of times out of ten that *S* was judged louder than that same other sound under the same conditions as before except that the latter sound received less attention.

larger than the roman, which means that a sound is judged less loud when simultaneously the attention is voluntarily directed to something else. If these results be accepted then without doubt it is true that *decrease of attention involves decrease of intensity of sound.*[1]

If so, then we have in this principle the means to explain the well-known phenomenon called the 'temporal error,' namely, that in comparing two successive sensations the

[1]Heymans, also, states that attention to an inhibiting psychic process increases its inhibitory power. *Ibid.*, 53, 1909, 402.

second usually is overestimated. It is obviously due to this that after the subject receives the first stimulus he awaits the second with stronger expectant attention.[1] Similarly, we can readily understand from the principles developed in the above four series why it is true that small differences between weights are much harder to discriminate when simultaneously than when successively given. The sensations from simultaneously given weights inhibit each other and thus forestall anything like the precise discrimination possible with successive sensations.

Series 6.

The Power of Voluntary Attention to Overcome Inhibition.

§15. *Procedure.*—Since in Series 1 and 2 it had been shown that a sound or pressure sensation may inhibit a pressure or sound sensation it suggests itself to inquire whether by an extraordinary effort to concentrate on the sensation judged the inhibiting influences of the distractor might not be neutralized. One set of observations (80) was taken with subject 7 and two sets with subject 9, sounds being judged. More would have been taken but for derangements in the apparatus and batteries. The following table shows what was done in the case of subject 7.

	Sound First Given.	Second Sound.
A	5	6.7
B	6	6.7
C	7	6.7
D	8	6.7
a	5/300	6.7
b	6/300	6.7
c	7/300	6.7
d	8/300	6.7

The sign /300 indicates when a 300-gr. weight was given with the first sound. The subject was directed to concentrate very hard on the sound that was accompanied by the pressure.

§16. *Results.*—The results for 80 observations with subject 7 were: A+B+C+D=17 and a+b+c+d=14. These

[1]Sometimes, however, subjective conditions are such as to bring heavier attention and corresponding overestimation to the first stimulus, but this is seldom found to occur for a large number of times in succession.

numbers represent the number of times the second sound was judged the louder. For subject 9 the results for 2 sets of 80 observations each were respectively A+B+C+D=26 while a+b+c+d=27 and A+B+C+D=24 while a+b+c+d=18.5.

These figures mean that the presence of the pressure simultaneous with the first sound did not succeed in causing the second sound to be judged louder more times than when the pressure was absent. In other words the extra concentration on the sound sensation prevented it being diminished by the pressure sensation.

This same compensation was demonstrated by some few observations on pressure sensations with sounds as distractives. 100 judgments by subject 8 and A+B+C+D+E=16.5 while a+b+c+d+e=14.5. A single test made with subject 7 also brings out the augmentor effect of voluntary attention. For this subject we take Series 3–C together with 3–D in Table VII. the figures A+B+C+D+E=52.5. This represents the number of times a pressure sensation was judged heavier than others when the sound of the buzzer occurred with the latter. We found that this number would be increased if the attention was voluntarily directed to the sound. Now a hundred tests were made with the direction to concentrate hard on the first pressure, with which the sound came, and the results were a+b+c+d+e=35.5. In short, voluntary concentration on the first pressure decreases the number of times that the second pressure is judged heavier.

Hence the results indicate that *the influence of an inhibiting agency may be neutralized by the voluntary attention.* It seems perfectly permissible to infer that the same thing could under favorable conditions be done by the involuntary attention, and that in general it it true that *just as decrease of attention involves decrease of intensity of a sensation, so conversely does voluntary or involuntary concentration involve increase of intensity.*

Series 6.

Inhibition of Pressure Sensations by Other Stronger Pressure Sensations.

§17. *Apparatus and Procedure.*—The writer had commenced his investigations on the inhibition of sensations with a series in which was tested the effect of lowering a 200-gr. weight over a pulley onto the left hand upon the strength of other pressure sensations on the right. The results were not the same for all the subjects, seeming to show an inhibition of the sensations of the right hand for some subjects and an augmentation for others. At that time, however, he did not understand the necessity of keeping the time periods of the inhibiting sensation and of the inhibited sensation in a certain constant relation. Hence it seemed desirable to repeat the test under more exact conditions.

The pressure sensations judged were produced on the right hand with the same apparatus as in Series 1 and the method was the same, except that the pressures lasted only two seconds instead of four. A special arangement was used for pressures on the left hand as shown in Fig. 4. The sub-

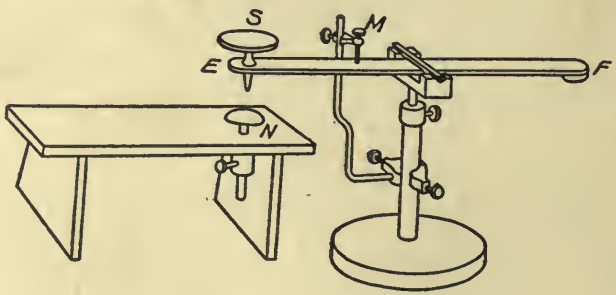


Fig. 4.

ject's left hand was placed palm downwards on the rest *N* and a cork disc again of about 15 mm. in diameter placed upon it. Now the screw *M* was turned down until the pin under *S* was in light contact with the cork. The pan *S* was loaded with a 300- or a 500-gr. weight. The fingers of the operator continually pressed down the lever *E–F* at the point *F*, so that he had but to remove his fingers in order that the

300- or 500-gr. pressure might be applied to the subject's hand.

Great effort was made to apply the pressures to both hands as nearly simultaneously as possible, and though the subjects asserted that they were often apperceived as not simultaneous, we may nevertheless reasonably assume that the success was in the long run satisfactory. Table X. shows the weights used for each subject.

TABLE X.

	Subject 3.		Subject 8.		Subject 1.		Subject 4.	
	First Wt. Given (gm.).	Second Wt. Given (gm.).	First Wt. Given (gm.).	Second Wt. Given (gm.).	First Wt. Given (gm.).	Second Wt. Given (gm.).	First Wt. Given (gm.).	Second Wt. Given (gm.).
A	22	20	18	15	18	15	21	17
B	20	22	15	18	15	18	17	21
C	22	20/300	18	15/500	18	15/500	21	17/500
D	20/300	22	15/500	18	15/500	18	17/500	21
E	22	24	18	21	18	22	21	26
F	24	22	21	18	22	18	26	21
G	22	24/300	18	21/500	18	22/500	21	26/500
H	24/300	22	21/500	18	22/500	18	26/500	21

The signs (/300) and (/500) indicate the application of pressure of 300 or 500 gr respectively. Otherwise the explanation of this table is the same as that of Table I

§18. *Results.*—The results for subjects 7, 8 and 1 show that the 500-gr. pressure did have a predominantly inhibitory effect on the sensations of the right hand. With subjects 3 and 8 this effect was clear from the first. With subject 1 some observations were previously taken where left-hand pressure 200 gr. and at another time left-hand pressure 400 gr. The results were inconclusive. This seemed possibly explanable because subject 1 made such restless efforts to judge the weights with the greatest accuracy and hence perhaps by frequent concentration on the right hand neutralized the inhibiting influence of the pressure on the left. Presuming that this might be the explanation of the case, the operator concluded that it would be necessary to have the weight on the right hand sufficiently heavy to manifest its influence in spite of the restless shiftings of the attention, and therefore he selected the 500-gr. weight. The results then showed, as previously remarked, that this weight had a marked inhibitory influence on the right-hand sensations.

As to the explanation of the results of subject 4 the writer is somewhat in doubt. That subject expressed a great lack of confidence in his judgment in this series, and the operator had similar misgivings. It is possible that the

TABLE XI.

		Subject 3.		Subject 8.		Subject 1.		Subject 4.	
Series 6–A.									
A	*C*	7	*10*	5	7½	8	*9*	3½	*0*
B	*D*	8	*8*	9	*10*	9	8½	7	3½
A+B	*C+D*	15	*18*	14	17½	17	17½	10½	3½
E	*G*	5	*10*	1	*6*	3	*6*	0	*0*
F	*H*	5	*6*	6½	*7*	½	*4*	2½	*1*
E+F	*G+H*	10	*16*	7½	*13*	3½	*10*	2½	*1*
A+B+E+F	*C+D+G+H*	25	*34*	21½	30½	20½	27½	13	4½
Series 6–B.									
A	*C*	5	*7*	3½	*9*	6½	6½	6	*4*
B	*D*	8	*8*	9½	*7*	7	*5*	8	4½
A+B	*C+D*	13	*15*	13	*16*	13½	11½	14	8½
E	*G*	2	*8*	1½	*10*	2	6½	1	*0*
F	*H*	3	*8*	3½	*7*	3	*3*	5½	7½
E+F	*G+H*	5	*16*	5	*17*	5	9½	6½	7½
A+B+E+F	*C+D+G+H*	18	*31*	18	*33*	18½	*21*	20½	*16*
Total sum	*Total sum*	43	*65*	39½	63½	39	48½	33½	20½

The explanation of this table is the same as that of Table II., except that the distractive used here is a simultaneous pressure of 300 or 500 gr., on the left hand instead of a sound.

strong pressure on the left hand exercised something of the same inhibitory influence in his case that it did with the others, for he reported that when the sensations from the right hand were faint they were sometimes almost crowded out of consciousness by the left-hand pressure. The subject stated that the pressure on the left hand markedly reduced the clearness of the sensations of the right hand. Upon being told of the results he thought them probably due to this unclearness coupled to an associated notion of heaviness whenever the left-hand pressure came. Other explanations, also, would be conceivable, but they only would introduce speculation.

At any rate so much was found to be true in this series: *that the intensity of the pressure sensations of moderate strength*

may often be diminished by other stronger pressure sensations.[1]

§19. *Conclusion.*—The results of our experiments have been to show that sound sensations of moderate intensity may be inhibited by simultaneous strong pressure sensations, that pressure sensations of moderate intensity may be inhibited by other simultaneous sensations either of sound or of pressure, that increase of attention to the inhibitor increases its effect, while increase of attention to the other sensation decreases the inhibition. This means that in the case of simultaneous sensations what is commonly known as effective distraction of the attention consists of the inhibitory influence which one sensation exerts upon another; and that degree of (subjective) intensity of sensation is a function of degree of attention. And though Heymans does not take up any special discussion of this last proposition, it can be deduced also from the facts he develops and demonstrates: a sensation to which there is adequate stimulus rises above the threshold in proportion as inhibition of it by other simultaneous sensations is absent. In other words, its intensity increases in proportion as the absence of other sensations permit the stimulus to be more and more attended. In short, the experiments of Heymans and Bentley taken together with our own seem to prove that degree of intensity of sensation is a function not of amount of objective stimulus alone, but of the latter taken together with the influence of other simultaneous sensations.

[1]We need to record in passing that questions about 'clearness' brought no unanimity of answer from the subjects, as previously stated; but it is interesting to note that in Series 4 and 5 where the attention was voluntarily placed on the inhibitor and the intensity of the other sensation much reduced as shown by the reports, yet the two subjects (6 and 7) who had the greatest skill in thus placing their attention agreed that for the most part the sensation not attended to was practically as clear as that upon which attention was directly placed. It was not our attempt, however, to make systematic and exhaustive investigation with reference to clearness and attention; but it seems well to throw out the suggestion here that our conclusion that decrease of attention brings decrease of intensity to sensation does not necessarily conflict with such facts as that distraction by noise during the process of addition brings decrease of clearness to the mathematical material attended to. (Geissler, 'The Measurement of Attention,' *Amer. Jour. of Psy.*, 20, 1909.)

THE RÔLE OF PITCH IN RHYTHM

BY HERBERT WOODROW, PH.D.,
University of Minnesota

IMPORTANCE OF QUANTITATIVE METHODS IN THE STUDY OF RHYTHM

Since the appearance, over sixteen years ago, of the pioneer work of Meumann[1] and of Bolton,[2] psychological literature has been enriched by very numerous researches dealing by means of experimental methods with the conditions of rhythm. Among the rhythmical influences most thoroughly studied under this heading have been the relative intensity of the different members of the series of stimuli which gives rise to the consciousness of rhythm, the rate of succession of these members of the stimulus series, their relative and absolute duration, and their pitch. The typical procedure in the investigation of these rhythmical factors has been to present a series of sounds or other stimuli, some of which are longer, more intense, or of different quality than the others, and to ask the observer to describe his impression of the series, especially as regards the way in which the stimuli appear to be grouped. Such a procedure is characteristically a psychophysical one, just as much so as when two weights are presented to an observer for him to lift, and then to describe his impression of them, with special reference to their relative heaviness. In both cases the investigation is one of relationship between certain conditions existing in the environment and the nature of the subject's consciousness under those conditions. Yet accurate psychophysical methods, methods capable of giving quantitative results, have not been used to any great extent in the study of the conditions of rhythm. Hitherto, neither the method of minimal changes

[1] 'Zur Lehre vom Zeitsinn,' *Phil. Stud.*, *9*, 1894, 264; 'Untersuchungen zur Psychologie und Aesthetik des Rhythmus,' *ibid.*, *10*, 1894, 249.

[2] 'Rhythm,' *Amer. Journ. of Psychol.*, *6*, 1894, 145.

nor that of constant stimuli has been used systematically in the direct investigation of rhythm, that is, in experiments in which the judgment rendered by the subject, instead of being one concerning the heaviness or the brightness of two or more stimuli, is one which concerns the way in which the stimuli appear to be grouped together, a judgment concerning the kind of rhythm of which the subject is conscious.

While much accurate and reliable information about rhythm has been obtained, the lack of quantitative results has hitherto constituted an important defect in our knowledge of the subject. By quantitative results are meant results which show, supposing the same *kind* of rhythm to be perceived under two different sets of conditions, in which case the rhythm is stronger, and how much stronger. One striking consequence of this lack of quantitative results concerning rhythm is, that all data so far obtained on the result of the combination of differences in duration of the members of the stimulus series with differences in either intensity or pitch are of little value; for evidently, the effect of a combination of two or more factors will depend not only on the direction, but, also upon the magnitude of the effect produced by each factor when acting separately. Both the direction and the magnitude of a resultant force depend upon the magnitude as well as the direction of the components. This elementary law, which applies in psychophysics and in psychology as well as in physics, makes it impossible to predict anything very definite concerning the effect of the combination of two or more rhythmical influences, especially influences of different direction, unless there is first known something about the magnitude of the effect of the separate influences. The importance of such quantitative knowledge is well shown, by its absence, in such cases as the following.—Bolton found that for seven subjects out of eight, in case every second sound was longer as well as louder, the longer and louder sound came at the end of the two-membered group.[1] But this result can not be generalized; as I have often found that making every second sound longer and louder *may* bring about the perception of a

[1] *Amer. Journ. of Psychol.*, *6*, 1894, 231.

very strong trochaic rhythm, one in which the longer and louder sound *begins* the group.[1] The result that will be obtained depends upon the direction *and magnitude* of the effect produced by the particular intensity and duration used, when each is acting alone. This, in turn, depends upon a large number of factors, including both the absolute and relative intensity and the absolute and relative duration of the sounds composing the rhythmical series.

Another instance of conclusions invalidated by the absence of quantitative results concerning the effect of the individual factors are the conclusions of Squire respecting the combination of differences in pitch with differences in duration.[2] No experiments were made to determine the effect on her subjects of the differences in duration alone. The statement made concerning one subject, that pitch was not only a constant determinant, but also a more effective one, than time,[3] meaning by time the duration of the tones, is unwarranted, because it is not shown that pitch and duration are working against each other, since the effect of the differences in duration which were introduced was unknown. It is quite possible that the differences in duration had no effect whatever upon the rhythm, because the effect of the increase in duration, as such, was possibly cancelled by the increase in apparent intensity which accompanies an increase in duration.[4] This increase in apparent intensity, due to increase in duration,[5] was not eliminated by Squire's method of equating intensities, for this consisted in a comparison by the subject of the

[1] *Archives of Psychol.*, *14*, 1909, Table VI., 36.

[2] 'Genetic Study of Rhythm,' *Amer. Journ. of Psychol.*, *12*, 1901, 546–560.

[3] *Ibid.*, 556.

[4] With sounds of very short absolute duration, this increase in apparent intensity as the result of an increase in duration is very great. Consequently, with sounds of short duration, if every second sound is two or three times longer than the others, it will seem louder; and if the intervals between the sounds are equal, the rhythm will be trochaic. With sounds of sufficiently great absolute duration, and of equal objective intensity, if every second sound is two or three times longer than the others, and the intervals between the sounds equal, the rhythm will be iambic. An intermediate duration can be found where the two effects cancel each other.

[5] Kafka, 'Ueber das Ansteigen der Tonerregung,' *Psychol. Stud.*, *2*, 1907, 256; Sander, 'Das Ansteigen der Schallerregung bei Toenen verschiedener Hoehe,' *ibid.*, *6*, 1910, 142.

intensities when the tones of the different pitches were given for the same length of time, namely, three seconds. It is true that if pitch and duration, when they occur separately, were to work in opposite directions, it would not require quantitative results to determine merely whether a certain difference in pitch could overcome a certain difference in duration. But even supposing that pitch and duration, when occurring separately, had opposite rhythmical effects, quantitative work would be necessary to determine whether they continued to work against each other when occurring in combination. It could not be assumed, without experimentation, that the rhythmical resultant of two such factors is determined by algebraic addition, as in the case of two physical forces working in opposite directions.

Another consequence of the lack of quantitative methods of studying rhythm has been the appearance of a great many contradictions in results obtained from different individuals by the same experimenter. These contradictions are to a great extent only apparent, and arise from the fact that as long as quantity is not taken into consideration, any two magnitudes of opposite signs are apt to be considered as differing widely from each other. A two-membered rhythmical group is either iambic or trochaic. These two judgments seem at first sight to mean radically different things: and yet two persons, one of whom judges iambic and the other trochaic, with respect to the same series of sounds, may be very much nearer to each other in their manner of perceiving the rhythm than two persons both of whom judge trochaic,—just as the difference between minus and plus one is less than that between plus one and plus one hundred. I have found, using the quantitative method outlined below, that two persons who disagree with regard to the quality of the rhythm may sometimes be made to agree by an extremely slight change in the rhythmical series presented to one of them, whereas, a considerable change must sometimes be made for one of the subjects, before two of them, both of whom originally judged trochaic, can be made to get an *equally strong* trochaic. Now, judgments concerning one's consciousness of

rhythm, like any other psychological processes, are variable; and this variability detracts from the probability that a judgment made only once will be repeated under the same experimental conditions. In view, then, of the possibility of the existence of a very small difference between the significance of the judgments trochaic and iambic, and the variability of all judgments concerning rhythm, it is a highly unreliable conclusion, where we have only one judgment made by each of a number of individuals, some of whom judge trochaic and some iambic, that there is any significant difference between the individuals in question. To understand the import of this, it should be remembered that the procedure in many experiments on rhythm consists in placing before an individual a series of sounds and letting that series continue until the subject makes up his mind concerning its rhythmical nature. The series being continuous, and the subject knowing that it has not been objectively changed, even though there be some fluctuation in the subject's mind before he arrives at a decision, there is obtained from him only one judgment.

Such a difference as that just referred to in the judgments of a number of individuals is frequently met with. Thus, in the experiments Bolton made with the view of investigating the rhythmical effect of differences in duration of the sounds and of differences in duration combined with differences in intensity, in no case did all subjects agree, though the number of subjects ranged only from four to ten, and in one case 'the preferred rhythm' was preferred by only four subjects out of seven, and in another by two out of four.[1] That this contradiction in the judgments of individuals is due to the failure to employ quantitative methods, is indicated by the fact that, by using the latter I was able to generalize the rhythmical effect of duration in the case of thirteen subjects in such a way that there were no individual exceptions.[2] Again, Squire found that when she presented an evenly spaced series of sounds of equal intensity and duration, in which every second or every third was of a different pitch from the others,

[1] *Amer. Journ. of Psychol.*, *6*, 1894, 231.

[2] 'A Quantitative Study of Rhythm,' *Archives of Psychology*, *14*, 1909, 64–66.

that the subjects, though only seven in number, fell into three classes,—those who accented the high tones, those who accented the low, and those to whom highness and lowness were a matter of indifference.[1] Squire concludes from her experiments that for certain individuals pitch has great effectiveness as a determinant of rhythm.[2] The results presented below, however, obtained from thirteen subjects, show that had the same methods been used as those employed by Squire, about the same results would have been obtained; but show further, in a conclusive manner, that pitch cannot in the case of a single subject be a reliable or constant determinant of rhythm. The reason for the greater uniformity and the absence of individual exceptions in the results reached in the present investigation, is to be found in the use of quantitative methods. As will be explained later, the amount of rhythm occurring when differences in pitch only are present is always exceedingly small, so small that many judgments are necessary to establish its reliability. But when these many independent judgments are obtained, it is found that the direction and quantity of the rhythm vary about zero so closely and so much as though by chance that the final conclusion can be only that pitch differences do not determine the rhythm at all.

General Survey of Experimental Results on Pitch as a Condition of Rhythm.

It is unnecessary to review all the widely diverging opinions concerning the effect of quality in rhythm, as most of them do not even make any pretense of being based on experimental data. In 1894, Bolton expressed the prevailing opinion, which was practically without experimental foundation, as follows: "Qualities of sounds are quite as important as unifying elements as their time and intensity relations, and were quite as early regarded." [3] Since then, the only study throwing much light upon pitch as a rhythmical factor is that by Squire. The results that have been obtained on

[1] *Amer. Journ. of Psychol.*, *12*, 1901, 556.
[2] *Ibid.*, 549–558.
[3] *Amer. Journ. of Psychol.*, *6*, 1894, 162.

rhythmical speech are hardly pertinent here. It is indeed true that a relationship has been found to exist between accent and pitch, the accented sound having generally a higher pitch than the unaccented.[1] But the same is true of prose, and may be regarded merely as a matter of ease of enunciation.[2] It is also true that occasional observations have been made, incidentally and in a loose way, which suggest some effectiveness on the part of pitch as a determinant of rhythm. Such observations hardly require criticism after what has been said of the importance of metric methods in the study of rhythm. MacDougall observed that differences in the quality of sounds produced a change in the rhythmical impression, that is, a change in the apparent grouping; and consequently he regards quality as a determinant of rhythm.[3] These observations were made incidentally in the course of his investigation of the effect of intensity of sounds in rhythm, the differences in quality occurring merely as the result of imperfections in his apparatus. In describing this apparatus, which consisted in a device for dropping metal weights in succession from adjustable heights, he writes: "The hammers and anvils, though fashioned from the same stuff and turned to identical shapes and weights, could not be made to ring qualitatively alike; and these differences, though slight, were sufficiently great to become the basis of discrimination between successive sounds and of the recognition upon their recurrence of particular hammer-strokes, thereby constituting new points of unification for the series of sounds. When the objective differences of intensity were marked, these minor qualitative variations were unregarded; but when the stresses introduced were weak, . . . they became sufficiently great to confuse or transform the apparent grouping of the rhythmical series; for a qualitative difference between two sounds, though imperceptible when comparison is made after a single occurrence of each, may readily become the subconscious basis for a unification of the pair into a

[1]Miyake, 'Researches on Rhythmic Action,' *Studies from the Yale Psychological Laboratory*, 1902, 1–48.

[2]Squire, *op. cit.*, 557.

[3]Psychol. Review, Monog. Sup., *4*, 1903, *Harvard Psychol. Stud.*, I., 317.

rhythmical group when several repetitions of them take place."[1] MacDougall says nothing as to the kind of effect a higher or lower pitch has, merely that difference in quality may change the expected grouping. But it is certain, if the different weights, when dropping from the same height, produced different qualities of sounds, that they also produced sounds of different loudness; so that when the difference in height was small, the hammer dropping the lesser height may in some instances have produced the louder sound.

Squire, in a carefully conducted investigation, came to the conclusion that for the majority of her subjects pitch was a determinant of rhythm. "While there was no general constancy in the accentuation of the high or low tones, *i. e.*, no constancy for *all* individuals, there was a marked constancy in the interpretation of the *same* individual throughout the different series given."[2] As a matter of fact, Squire's tables, when studied in detail, show that for the majority of her seven subjects there was no constancy in the interpretation of the different series.[3] Her conclusion, that with some subjects pitch is a more effective determinant of rhythm than time, has already been referred to. Squire believes, however, that quality as such is not an independent determinant of rhythm, but owes its effectiveness to associations. "It can be considered as a substitute for or as an intensification of intensity,—whether because of strain sensations common to both, or because of associations which, reduced to their ultimate grounds, are of an intensive character."[4] The introspections which Squire has recorded, rather than bearing

[1] *Ibid.*

[2] *Op. cit.*, 549.

[3] *Op. cit.*, 550–555. Subjects W.M., E.P., B.B. and I.M. did not maintain throughout the different combinations groupings beginning with either the higher or the lower pitched sound, but sometimes obtained groups beginning with the higher pitched and sometimes groups beginning with the lower pitched.

[4] *Op. cit.*, 558. Compare with the opposite view of Ettlinger: "Beim Wechsel verschiedener Intensitaeten draengt sich die rhythmische Bewegungsvorstellung viel zwingender auf, als beim Wechsel der Tonhoehe. . . . Es waere doch irrtuemlich, die stellvertretende Wirkung der Rhythmusurachen so aufzufassen, als wuerden die anderen erst durch Vortaeuschung der Intensitaetsstufen hindurch Ursachen des Rhythmus." *Zeitschrift f. Psychol. u. Physiol. der Sinnesorgane*, *22*, 1900, 180–181.

out the interpretation she gives to her results, as being due to associations with intensity, make this interpretation impossible. For example: F.W., "who was fully determined to accent the higher," gives the introspection: "The two tones take the accent equally well. . . . I can change the accent at will": "Higher is first, it is longer, a dragging tone. The lower is more concise, it is accented." (This subject is one of those who is said to have mistaken an *a* of 453 as higher than a *c* of 512.) Now, how can it be argued that the higher pitched sound always came first in the group because of associations with intensity when the introspections show that the lower took the accent as easily as the higher? And after all, was not all influence due to apparent intensity or to associations between pitch and intensity eliminated by the method of equating the intensities of the sounds at the beginning of the experiment? This consisted in regulating their intensity so that the sounds of different pitch seemed equally loud to the subject. No associations with intensity are suggested which would be effective in rhythm, which would not be equally effective while the sounds were being equated as regards apparent intensity and so eliminated; and it seems likely that if such associations had existed, they would have been mentioned in the introspections, as the latter were, apparently, the object of considerable care.[1]

Squire's method, which consisted in always using series in which the sounds were separated by equal intervals, is not a reliable one, as has been already explained. With any one

[1]Squire's interpretation of her results shows considerable similarity to Meumann's position. Meumann, while believing that differences in quality may be regarded as independent causes of rhythm, (*Phil. Stud.*, *9*, 1894, 305) the same as any periodically recurring differences, says, that it seems to him that the importance of certain tones in rhythmical series depends chiefly on the stronger beat and the retarded tempo (*ibid.*, 307). He explains the aid to 'subjective' rhythm which he supposes to be produced by differences in pitch as due solely to apparent differences in the intensity and duration resulting from the differences in pitch. Melody comes in as a rhythmical factor, first, as a result of the greater intensity and duration of certain tones, and second, from 'Innere Zusammenhang.' The latter, in musical rhythm, apparently corresponds with the factor which Meumann distinguishes in poetical rhythm as the "Logischen Zusammenhaengen zu Saetzen." Warner Brown says that it is preposterous to suggest the latter as a rhythmical factor. ('Time in English Verse Rhythm,' *Archives of Psychology*, *10*, 1908, 72.)

series, only one judgment was obtained. It is true that several different combinations were used, such as series in which every second was higher, every third higher, or every third lower. That some of the subjects kept to the same sort of grouping throughout all the different combinations is not however out of keeping with the interpretation, necessitated apparently by the results presented below, that pitch is not a reliable determinant of rhythm. After the subjects had once decided that the higher ended the group, in one of the series, some of them would naturally be inclined to give the same judgment in other series, unless a series was given in which it was plain to them that the higher sound ended the group, so that they would be awakened to the necessity of making new decisions independent of their past ones. The results below show that the amount of rhythm obtained, when no differences but those of pitch exist in the sounds, is always so small, that, in view of the variation existing between successive judgments, when made independently, a large number of independent judgments would be necessary to establish a significant difference between those who judge higher first and those who judge higher second, concerning the same rhythmical series.

Squire says that, one great difficulty in interpreting her results was, the determination whether subjectively high and low corresponded to, objectively high and low. She states that when confusion of judgment was found in the three-group, she presumes that it was present also in the two-group.[1] But it is nowhere explained how this confusion was straightened out. That it never was, is indicated by the almost incredible fact that five of her seven subjects are said to have either been unable to distinguish the difference between *a* of 453 vibrations and *c* of 512 vibrations, or, to have mistaken the higher for the lower,[2] and one subject is said to have found no difference in pitch between a *c* of 256 vibrations and an *a* of 453 vibrations. As these were the pitches used in Squire's investigation, how could she possibly hope

[1] *Op. cit.*, 557.

[2] *Op. cit.*, 550–555. Subjects B.B., C.S., E.P., I.M., and F.W.

to determine the effect of pitch in rhythm with such subjects?

This brief survey of the actual facts at hand concerning the rôle of pitch in rhythm is sufficient to show that my own conclusion, that pitch is not a determinant of rhythm, is not at variance with any well established position. The conclusions of Squire are unwarranted as well as undecided, and this, together with the negative effect of pitch on the estimation of time intervals,[1] enables me to say that my conclusion is, if anything, more in keeping with previous knowledge than at variance therewith.

Apparatus and Procedure.

The apparatus that I have used for the production of rhythmical series of sounds, while complicated, has several advantages over that used by previous investigators. It permits of independent variation of the duration of intervals and sounds, of the rate at which the series is run off, of the number of sounds contained in a measure, of the intensity and pitch of any sound, of accurate measurement and control of all these variables, and of the ready substitution of electrical shocks, as stimuli, in place of sounds.[2] The sounds used in the present research were produced by passing an electric current, made intermittent by means of an electrical tuning-fork, through a telephone receiver.[3] The diaphragm of the telephone receiver vibrates at a rate identical to that of the tuning-fork used. A Meumann's time-sense machine[4] was used for the purpose of making and interrupting the intermittent current. The time-sense machine permits of

[1]Shaw and Wrinch, *University of Toronto Studies, Psychological Series*, 1899. These authors, in a carefully conducted set of experiments, sought to determine by means of tapping movements "the degree of accuracy that obtains in the reproduction of complex rhythmically arranged groups of intervals of unequal length." The length of the intervals was found to be unaffected by the differences in the pitch of the tones. Sears, *Amer. Journ. of Psychol.*, 1902, 33, states that pitch of tones has no effect on the relative length given to intervals by the performing musician.

[2]For a more detailed account of this apparatus, with diagram and photograph, see 'A Quantitative Study of Rhythm,' *Archives of Psychology*, *14*, 1909, 12–29.

[3]This arrangement for producing sounds of different pitch has been recently used by Paul Sander, 'Das Ansteigen der Schallerregung bei Toenen verschiedener Hoehe,' *Psychol. Stud.*, *6*, 1910, 1–38.

[4]Described in *Phil. Stud.*, *12*, 1896, 142–152.

variation in length of tones and the intervals between them, variations which to a large extent may be made independently of each other. The durations of intervals and tones were measured by placing in circuit with the telephone receiver a time-marker which wrote on a smoked drum alongside of a tuning-fork of 100 vibrations per second. The time-sense machine was run by an ordinary fan motor and several speed reducers. The rate of rotation obtained in this manner was more constant than that obtained by means of the Helmholtz motor.[1] The intensity of the sounds produced by the telephone receiver was regulated by means of shunts and resistances. To obtain sounds of different pitch, the circuits passing through different contacts on the Meumann's machine were passed through tuning-forks of different pitch. In this way, every second or every third sound could be made higher or lower than the others. The three tuning-forks which were used had pitches of 440, 250, and 50 double vibrations per second. Each tuning-fork was provided with two platinum contacts, one for the circuit which kept the fork vibrating, and one for the sound circuit which passed through Meumann's machine and the telephone. With this arrangement, there was no noticeable click either at the start or end of the sounds. Both of the tuning-fork contacts required very careful adjustment, but it was found possible to produce fairly musical tones, of distinct pitch. Resonators were unnecessary, and were not used. In purity, the sounds were about equal to voice tones as produced by a good phonograph. One of my subjects was a musician, and she was able to identify the pitch of the different sounds very closely, for instance, calling the sound produced by the 250 fork *c'*. She had, further, no difficulty in reproducing by voice either of the two higher tones used. All of the subjects, as well as Professor Pillsbury and Dr. Shepard of the psychological department of the University of Michigan,[2] said that the tones had a distinct

[1]For measurements of the constancy of the rate of rotation with the Helmholtz motor, see *Archives of Psychol.*, *14*, 1909, 16.

[2]This research was carried on, during the summer of 1909, in the psychological laboratory of the University of Michigan, all the ample facilities of which Professor Pillsbury was kind enough to throw open to me. I take this occasion to thank Professor Pillsbury and Dr. Shepard for their invaluable help.

pitch; and those who had had any experience with musical instruments said they thought they could tune an instrument to any of the tones used. One of them tuned a tuning-fork to the sound of 50 double vibrations per second, and missed it by 4 vibrations per second. One subject said of the 440 tone that it sounded like a tuning fork, and another, of the 50, that it resembled in quality the tone of a violincello. Further, though the majority of the subjects were not especially musical, none of them ever confused the pitch of the tones.

The sounds of different pitch were equated for intensity by the subject at the beginning of each sitting. This was necessary, as between sittings the condition of the platinum contacts would change slightly as the result of slight jars to the floor of the room, oxidation, etc. Further, during the middle of each sitting, the rhythmical series was stopped and the tones given in unrhythmical succession to see if they still appeared of equal intensity to the subject. Or, if at any time the subject suspected that the tones were not of the same intensity, the rhythm was stopped and he was given a chance to readjust their intensity. The method used in equating the intensities was about the same as that used by a musician in tuning one tone to another, except that the tuning was for intensity and done by sliding back and forth the slide of a wire resistance-coil. During this act of adjustment by the subject, the tones were not given fast enough nor regularly enough to permit of any rhythmical effect. Thus, throughout the whole investigation, the different tones used all had equal apparent intensity, and the effect of pitch on apparent intensity was eliminated.

The method used in the present investigation rests upon the fact that it is possible to pass from one rhythmical grouping to another by changing the relative duration of the intervals between the sounds. Thus, a rhythm in which the louder, longer, or higher pitched sound begins the group, may be changed to one in which that sound ends the group by increasing sufficiently the interval following that sound, and vice versa. In twenty-five subjects, I have found no exception to this statement. We pass in this way by small

steps from rhythmical groups beginning with a certain sound, to groups ending with that sound, and through a zone where the tendency towards the two forms of grouping is equally strong, or where the tendency to group disappears. The middle point of this zone may be termed the rhythm indifference point. If the rhythm indifference point occurs where all the intervals are objectively equal, then any differences which may exist in the objective duration, intensity, or quality of the sounds are obviously not exerting any effect towards rhythmical grouping. But if, when the intervals between all the sounds are objectively equal, grouping is still perceived, the grouping can not have been occasioned by objective differences in the intervals, and must be due to the presence of other rhythmical factors. The amount of this grouping effect, due to rhythmical factors other than unequal intervals, can be measured by determining the amount by which, *measuring from the point at which all intervals are equal*, the internal intervals, or intervals within the group, must be increased or decreased with respect to the external intervals, or the intervals separating the groups, in order to cause a disappearance of the rhythm, that is, a disappearance of apparent grouping. In short, the amount of rhythm is measured by the amount of change that has to be made in the intervals in order to cause a disappearance of the rhythm. This may be determined either by the method of minimal changes or by that of constant stimuli. In the present investigation, the method of minimal changes was used exclusively.

The subject sat in a room separate from that containing all apparatus but the sound-producing telephone and a sliding resistance, by means of which the subject regulated the intensity of the sounds, in the manner already described. The subject was first given a series of sounds concerning the rhythmical nature of which there could be no doubt, one which he could judge to be plainly iambic, or composed of groups with the higher pitched sound coming last, or with the higher pitched first, etc. This series was allowed to run along uninterrupted until the subject indicated by a bell

signal that he had arrived at a decision. If the subject signalled that he had perceived the rhythm as composed of groups beginning with the louder sound, the interval following the louder sound was slightly lengthened, and that preceding shortened by the same amount. This second rhythm was then given and continued until the subject came to a decision. This procedure was continued until finally the subject began to judge the group to end with the louder sound, in other words, until the rhythm had shifted over from trochaic to iambic. The size of the steps, that is, the amount by which one of the intervals was shortened and the other lengthened was either 3, 4, or 5 times 0.0083 sec., the latter value being that of one degree on Meumann's machine, at the rate of revolution used; but the same sized steps were used throughout any one series. The usual precautions in applying the method of minimal changes were taken, as well as certain additional ones, made necessary by the peculiar nature of the material worked with.[1] In determining any one indifference point, a number of such series of minimal changes, going in both directions, was used.

Results.

The procedure above outlined is applicable to the investigation of variations in intensity, duration, pitch, or any combination of these factors, and of variations in rate. In order to bring out the significance of the data, presented below, on the effect of pitch, it is necessary first to indicate briefly the result obtained by using such a procedure, on the effect of intensity, duration, and rate. As regards rate, it was found that the amount of rhythm produced by objective differences in duration or intensity, measured by the difference in the intervals at the indifference point, remained constant, relative to the duration of one measure, in two-group rhythms, for rates varying approximately from one to four seconds for one measure.[2] Consequently, results

[1] See *Archives of Psychol.*, *14*, 1909, 20–29.

[2] By *measure*, is meant the time from any point in one group to the corresponding point in the following group, *e. g.*, when every second sound is longer, from the beginning of one of the longer sounds to the beginning of the following longer sound.

obtained at a rate of 1.5 seconds for one measure, the rate used in the present investigation, hold, proportionally to the rate, for series run off either slightly faster or several times slower. At rates slower than one measure in four seconds, there was a gradual falling off in the amount of rhythm present, until at a duration of the measure of from seven to ten seconds, it had nearly or quite disappeared. The fastest rate used was one sound every fourth of a second. There was no tendency whatever for rhythms which were trochaic at the slow or medium rates to become iambic with the fast rates, as claimed by Wundt.[1] With rates of one second or less for a two-beat measure, the rhythm was in every case just as strongly trochaic, or else more so, than with rates of from 1.5 seconds or longer for a two-beat measure.[2] Regarding intensity, it was found that in rhythmical series in which every second or every third sound was more intense, that with an increase in the ratio of the intensity of the louder sound to that of the weaker, there is an increase, first rapid, and then slow, in the tendency of the more intense sound to begin the group. As regards duration, with an increase in the duration of the longer sound to that of the shorter, there is an increase in the tendency of the longer sound to end the group or a decrease in its tendency to begin the group. I had one subject with whom there was only such a slight decrease in the tendency of the longer sound to begin the group, as its length was increased, that I was not certain but that he offered an exception to the above generalization. I have since been able to experiment further with him, however, and find that with sounds of greater absolute duration, he conforms very decidedly to the above generalization. These new results, presented in Table I., not only indicate in a typical manner the effect of increasing the duration of every second sound, but also confirm my conclusion concerning the effect of an increase in *absolute* duration, namely, that with a constant ratio between the duration of the longer and shorter sounds, as their absolute duration increases, there is a decrease in the tendency of the

[1] 'Grundzüge der Physiologischen Psychologie,' 5th ed., 1903, Vol. III., 99.
[2] *Archives of Psychol.*, *14*, 1909, 34–37.

longer sound to begin the group or an increase in its tendency to end the group. This is brought out by the fact that in the previous work with this subject, in which the shorter sound had always a duration of 0.13 second, no matter if I made the longer sound 6.5 times as long as the shorter, so long as the intervals between the sounds were objectively equal, the rhythm never quite went over to iambic, but merely became somewhat less pronouncedly trochaic. In Table I., however, it is shown that when the absolute duration of the shorter sound is increased to 0.45 second, that by the time the ratio of the duration of the longer to the shorter sound has reached three to one, the rhythm is slightly iambic, and becomes markedly so with a ratio of four to one. The change from trochaic to iambic grouping is indicated in the table by the change in sign of *A–B* from plus to minus.

Table I.

The Effect of Duration on the Iambic-Trochaic Indifference Point.

Subject, Dy. Duration of shorter sound, constant, = 0.45 second. Duration of one measure, constant, = 3.0 seconds.

Longer.	Before.	After.	*A–B.*	*MV.*	*N.*
.60	.916	1.034	+.118	.04	18
.80	.846	.904	+.058	.03	16
1.20	.682	.668	−.014	.02	18
1.60	.515	.435	−.080	.02	15

N = number of series of minimal changes used in the final determination of any one indifference point, the results of the separate series being averaged.

MV = mean variation in the indifference points obtained by the different series of minimal changes.

After = duration in seconds of interval after longer sound, and Before = duration in seconds of interval before longer sound, *at the rhythm indifference point.*

A–B = interval *after* minus interval *before*. When *A–B* is positive, it shows that with equal intervals the rhythm is trochaic (longer sound beginning the group) and the amount of *A–B* shows how much the intervals had to be changed from objective equality in order to arrive at the indifference point, *i. e.*, in order to destroy the trochaic rhythm. When *A–B* is minus, it shows that the rhythm is iambic with equal intervals, etc.

Longer = duration of longer sound.

After the explanations that have already been given in connection with Table I., the following tables on the effect of differences in pitch will be readily understood. The duration

of one measure is the time that elapses from the beginning of one of the higher pitched sounds to the beginning of the first succeeding sound of that same pitch. In the case of every third sound lower and the other two higher, for example, it is the time from the beginning of any one of the lower to the beginning of the following lower. The duration of the whole measure remains the same throughout the investigation of any given combination of pitches. Under *Higher* and *Lower* are given the pitch of the higher and the lower tones. By *A–B* is meant the interval after the highest pitched minus that after the lower pitched, each interval being given the length it is found to have, on the average, at the rhythm indifference point. By intervals, is meant the intervals of silence between the sounds. All intervals and durations are expressed in seconds. *A–B* expresses the difference from objective equality of intervals that is necessary in order to destroy whatever rhythm is produced by the differences in pitch. The indifference point is determined, as above described, by passing by small steps, back and forth, from rhythms with the highest sound beginning the group to rhythms with the highest sound ending the group, and taking the point where both groupings seem equally natural or where no grouping at all is obtained. The number of such *series* of small steps, or minimal changes, used in determining any one indifference point, is indicated under *N*. Each series of minimal changes, of course, passed through the rhythm indifference point, and so gave a value for *A–B*. If, then, for example, sixteen series were used in any one case, the value of *A–B* given is the average of the sixteen values obtained by the sixteen series; and the *MV* given is the mean variation of these sixteen values of *A–B* about their average.

Some explanation is necessary as regards the sign of *A–B*. This magnitude, it must be remembered, shows how much the intervals need to be changed from objective equality in order to arrive at the point where grouping disappears, and so is a measure of the grouping effect of whatever rhythmical conditions may be present in the stimulus series. Now, the change from objective equality may have to be made in either of two

directions. If every other sound is louder, the interval after the louder sound has to be lengthened to arrive at the rhythm indifference point, so that when that point is reached, the interval after the accented sound is longer than that before it; in other words, *A–B* is positive. It is of course purely arbitrary which interval is subtracted from the other. I have reckoned the difference between the intervals by subtracting the interval before the loudest, longest, or higher pitched sounds from the others. In Tables II., III., and IV., a positive value of *A–B* means a group-beginning tendency on the part of the higher pitched sound; a negative value of *A–B*, a group-ending tendency on the part of the higher pitched sound. Thus, when *A–B* is positive, it means that the higher pitched note is holding the following lower pitched note or notes to it, so that in order to destroy the rhythm the interval immediately following the higher pitched has to be lengthened as compared with the interval preceding it. When

Table II.

The Effect of Pitch in Two-Group Rhythms.

Duration of one measure, constant, = 1.5 seconds. Duration of every sound = 0.2 second. Duration of intervals, when equal, = 0.55 second.

Subject.	Higher.	Lower.	*A–B.*	*MV.*	*N.*
F Rr	440	250	−.04	*.04*	16
Ce			−.02	*.03*	16
Hr			−.02	*.03*	19
Dy			.00	*.03*	44
B Rr			.00	*.02*	19
Cr			+.01	*.05*	45
Mr			+.02	*.02*	18
Ww			+.02	*.01*	37
Bs			+.03	*.01*	38
Be			+.04	*.03*	18
Ww	440	50	−.02	*.01*	18
Dy			−.02	*.02*	21
Ce			.00	*.03*	20
B Rr			+.02	*.03*	34
Bs			+.03	*.03*	22
Mn	250	50	−.02	*.02*	18
Bs			−.01	*.01*	14
Hn			.00	*.02*	19
Dy			.00	*.02*	20
B Rr			+.02	*.04*	22
Ww			+.02	*.01*	23

Table III.

The Effect of Pitch in Three-Group Rhythms.

Duration of one measure, constant, = 2.0 seconds. Duration of every sound = 0.13 second. Duration of intervals, when equal, = 0.53 second.

(*A*) *Groups of one lower and two higher pitched sounds.*

Subject.	Higher.	Lower.	*A–B.*	*MV.*	*N.*
Ww	440	250	+.04	.02	17
Bs			+.02	.06	13
Ce			+.02	.03	13
Dy			+.01	.02	27
B Rr			−.02	.02	18
Bs	440	50	+.02	.04	21
Dy			.00	.03	20
B Rr			−.02	.02	17
Ww			−.03	.02	15
Ce			−.08	.02	16

(*B*) *Groups of one higher and two lower pitched sounds.*

Bs	440	250	−.01	.05	18
Ww			.00	.01	17
B Rr			.00	.03	19
Dy			+.01	.02	20
Ce			+.04	.04	20
Bs	440	50	−.14	.03	14
Ww			−.06	.01	10
Dy			−.01	.02	30
B Rr			.00	.03	19

A–B is negative, it means that with equal intervals the grouping is lower note first and higher last, so that the interval preceding the higher must be increased in order to make the grouping disappear. In the case of groups of one low and two high tones, a positive *A–B* means that with equal intervals the two higher notes begin the group; a negative, that they end it. Similarly, in the case of groups of two low tones and one high one, a positive value of *A–B* means that with equal intervals the high tone begins the group; a negative, that the high tone ends it. Rhythmical groups of the form high-low-high or low-high-low were never obtained except at, or very near, the rhythm indifference point, where almost any rhythm that the subject tries for, or none at all, is equally natural.

In interpreting these results, it is necessary to keep in mind the results already referred to as obtained by the same

method in the case of intensity and duration, and also the significance of the value *A–B*. As already explained, the quantity *A–B* serves as a measure of the quantity of rhythm produced by whatever rhythmical factors are present in the series. In rhythmical series containing sounds of different intensities or of different durations, in many cases the quantity *A–B*, that is, the difference between the intervals preceding and following the louder or longer sound at the indifference point, was very large. In some rhythms produced by alternations of more intense with less intense sounds, I have found the interval after the more intense to be more

TABLE IV.

SHOWING FLUCTUATION IN THE VALUE OF *A–B* WITH DIFFERENT COMBINATIONS OF PITCHES.

Objective Form.		1	2	3	4	5	6	7	Av. *A–B*.	Av. *MV*.
Subject:	B Rr	.00	+.02	+.02	−.02	−.02	.00	.00	.000	.027
	Ww	+.02	−.02	+.02	+.04	−.03	.00	−.06	−.004	.013
	Ce	−.02	.00		+.02	−.08	+.04		−.008	.030
	Bs	+.03	+.03	−.01	+.02	+.02	−.01	−.14	−.009	.033
	Dy	.00	−.02	.00	+.01	.00	+.01	−.01	−.001	.023

The numbers given to the objective forms follow the order of Tables II. and III. Thus objective form no. 1 is an alternation of the two pitches 440 and 250; form no. 2, an alternation of 440 and 50; no. 3, an alternation of 250 and 50; no. 4, every third sound 250, the others 440; no. 5, every third sound 50, the others 440; no. 6, every third sound 440, the others 250; no. 7, every third sound 440, the others 50.

than 50 per cent. longer than that before it, at the rhythm indifference point. The average size, for four subjects, of *A–B* in the case of the greatest intensive differences I used was 40 per cent. of the interval *B*, which means that the interval before the accented sound was 0.50 second and that after it 0.70 second, at the rhythm indifference point. In rhythms produced by an alternation of a longer and a shorter sound, the interval before the longer had, in some cases, to be made over 100 per cent. longer than that following it, in order to arrive at the indifference point. In one case, it was 135 per cent. longer. The results obtained when only pitch differences were present offer a striking contrast. For two-membered groups, the greatest size obtained by *A–B*

was about 8 per cent., in once case +8 per cent. and in another —8 per cent., of the interval before the higher pitched sound. In 18 cases out of 21, it was less than 4 per cent. In both Tables II. and III., that is, in both two-membered and three-membered groups, *A–B* is zero more often than anything else. This means that more often than anything else no tendency towards either form of grouping existed when the objective intervals between the sounds were equal. In the remaining cases, *A–B*, and consequently the amount of rhythm found to exist when the intervals were equal, was very small. No one could expect the value of *A–B*, in view of its variability, to come out exactly zero in every case, even supposing that were its true value. Yet, 75 per cent. of the values of *A–B* found in Tables II. and III. are less than the size of the corresponding mean variations.

Moreover, not in the case of a single subject was there uniformity in the sign of *A–B*. This is shown in Table IV., which presents the results obtained on the five subjects who judged of more than one type of series. This table shows that not only was the amount of rhythm always small, but that the direction of the grouping effect varied for the same subject in the different pitch combinations. For the purpose of determining whether or not a higher pitched tone occurring every second or every third sound has a group-ending or a group-beginning tendency, it is legitimate to average the results obtained with the same subject in the different combinations. I have arbitrarily called the higher-beginning tendency the positive direction, and the lower-beginning tendency the negative. That a higher or lower tone has no general tendency in the case of any of these five subjects to begin or end the group is shown by the fact that the average of the pitch effect in the seven different combinations comes out in the case of each subject practically zero,—in every case it is less than 0.01 second and at least several times less than the average mean variation for all the different combinations. The result is the same, if, instead of considering as positive the higher-beginning tendency, we treat the results in any other way. For instance, we may consider the question

as to whether, in three-group rhythms, that tone which is of a different pitch from the other two, irrespective of whether it is higher or lower, has not either a group-beginning or group-ending tendency. But from whatever angle the results are viewed, the fact remains that the differences in pitch are always productive of practically no rhythm, and no general tendency whatever exists as to the direction of the insignificant amounts of it that are produced.

The very slight indications of rhythm obtained in many of the cases, I regard as not due to the action of pitch as a rhythmical factor, but as due to the variation in the measurements of a quantity whose true value is zero. Zero is the value most frequently found, and the one about which the others are rather symmetrically distributed. It is true that the difference between the averages for $A-B$ in the case of the same subject for different types of rhythm is great enough, in some instances, at any rate, to have some significance. There are probably certain constant errors entering in, which were different for the different types of rhythm. Such may have been certain specialized habits of motor response, or of attention, or errors in equating the intensities. Also errors in measurement and readings on the part of the experimenter. These might be different for each type of rhythm, as in nearly all cases all the work done with any one combination of pitches by any one subject was finished before work with the next combination was begun; and in any case, it was not difficult for the subject to recognize the type of rhythm used, as the three pitches used were all widely different. But the fact, that the differences existing between the quantities $A-B$ are not wholly without significance, makes it none the less certain, in view of the fact that the sign of $A-B$ changes for the different types of rhythm, that the rhythm is not determined in a reliable or constant way by the pitch differences.

I have used only seven different combinations of pitches and these all very simple ones. It might be argued that my conclusions would not hold, were other combinations used. While admitting this possibility, I wish to emphasize the

fact that I have used harmonious combinations of pitches as well as some that were not especially harmonious, with the same negative result in both cases. It seems to me extremely doubtful whether the more complex melodies would be found any more rhythmical than the combinations of pitches that I used, it being remembered that differences in duration as well as in real and apparent intensity are to be considered as having been entirely eliminated.

Conclusion.

Pitch, intensity and duration can no longer be looked upon as *stellvertretende* factors, any one of which may be substituted for either of the other two. The rôle of each in rhythm is radically different. Intensity has a group-beginning effect: duration, a group-ending effect: pitch, neither a group-ending nor a group-beginning effect.

EDITORIAL

WILLIAM JAMES

No one who knew William James can ever write of him in a wholly objective and dispassionate spirit. Nor can one cherish any hope of tranfusing into words the abundant richness of his wonderful nature. Any just estimate of the man would require for its accomplishment a soul as pure and fine and brave as his own. The final valuation of his work must be confided to another generation. These lines are devoted to a few brief reminiscences, and particularly to such as may remind us of the profound changes in our psychological thought and practice for which we are primarily indebted to him.

To be sure at the time of his death, James was generally regarded as a philosopher rather than as a psychologist. And a philosopher he was in the best sense of the word, a devoted and courageous seeker after the deepest truths of life. But he was also a psychologist, easily first among his countrymen, and to emphasize this fact is the peculiar privilege of this REVIEW, with the first number of which his name was associated as an editor.

We may begin our reminiscences with the publication of 'The Principles of Psychology' in 1890. Prior to that James had been writing occasional brilliant articles and essays, some of which appeared later in modified form as chapters in the book, still others coming to life again in the volume entitled *The Will to Believe*. But until the appearance of The Principles his influence was on the whole secondary and local. With the publication of this great work, which the writer believes to be altogether his most valuable and lasting monument (it has in it most distinctly the seeds of all his later philosophical development) a profound and radical change came over the scene.

Who does not remember the sense of glowing delight with which we first read the pages of the big, cumbrous, ill-bound and rather ill-printed volumes? It was like inhaling a rare, pungent mountain air, vital, bracing and almost intoxicating. To many of us of the younger generation the book was assigned as a text. We read it as one reads the most fascinating tale of a master—spell-bound and transported and yet withal feeling ourselves acquiring new powers, and gaining command of pregnant thoughts. Scores of other readers fared as did we and so it came to pass that almost over night James

became the recognized fountain head of the most original and most vigorous psychological thinking in our country. To get the perspective one must recall the conditions of our literature at that time.

In 1887 Ladd published his treatise on *Physiological Psychology* and Dewey his text-book on *General Psychology*. Two years later came Baldwin's *Senses and Intellect*. Signs were therefore not wanting of an intellectual ferment at work. But Ladd's volume was too technical to attract general attention, and the other two books, although favorably received, suffered under the suspicion of being philosophy rather than pure psychology, added to which both were addressed to the needs of college students and followed rather rigid lines of exposition. Prior to the appearance of these works, American students had been nurtured almost exclusively on Porter, McCosh, Hickok, Wayland, Haven, Hopkins, and their various followers. Not that all these authors deserve in any sense to be classed together, but simply that they represented in various degrees scientifically archaic and outworn methods in psychology, which were nevertheless still current in our colleges. At such a critical period appeared the epoch-making *Principles*.

It is difficult to appreciate how much that is now familiar and commonplace in psychological writing was introduced by James. But the simplest process of comparing the authors last mentioned with the writings of to-day abundantly exhibits the transition which has occurred and for which James more than any other one man is directly responsible.

Perhaps the most fundamental of all the doctrines he introduced and popularized was the physiological conception of habit as the basal principle of mental organization. He not only made this pivotal in his account of the associative processes of the intellect, but he exhibited it as the central feature in the development of the will. When a decade later he published his *Talks to Teachers*, he took it up again and made it cardinal for educational practice.

If the principle of habit may in certain ways be regarded as the most important interpretative principle running through his work and the one which enlisted the widest and most immediate interest —and general assent—it must in other ways share the honor with its complementary principle, the cerebralistic hypothesis of the conditions of thought.

This hypothesis is of course in no sense an invention of William James. Indeed, in many ways it simply formulates facts well known to the veriest way-farer. But no previous English writer on psychol-

ogy since Hartley's day had espoused this view so explicitly and at the same time made such constant practical use of it. James turns to it at every point for the explanation of mental phenomena, a practice which caused him to be branded by the orthodox as a dangerous materialist.

He shared with Lange the honor of revolutionizing the current view of emotion. The exposition of his theory rescued the subject at a stroke from the dull routine of purely descriptive psychology and, notwithstanding a constant fire of acute criticism, his position has substantially held its own up to the present day.

Following closely in the footsteps of Darwin, he made instinct an essential part of the study of the human mind in a way no other psychologist had done. He took a large, flexible, dynamic view of instinct which gave it a place in the very forefront of human life and so of human psychology, instead of relegating it to the limbo of 'left-overs' from our animal ancestry for which apologies must be made and moralizing indulged.

He wrote perhaps the only thoroughly entertaining account of reasoning that we have in the English language and yet managed to make clear much of what is technically most essential to know, if one would understand the actual workings of the mind. Despite their admirable analytic fidelity to detail, one has only to compare with James' chapter on 'Reasoning' in the *Principles*, certain of the contemporary German analyses of the process, to feel that he is in reality closer to the pith of the matter than they.

His account of space perception established the subject on a level which it had never before reached in modern English writings and thereby rendered a service of lasting value. It still remains, twenty years after it was written, one of the most valuable of store-houses for all the fundamental phases of the subject.

James made a conscious and systematic struggle for the recognition of the vague, the fugitive, the transitory, in consciousness, as over against the 'block-house' scheme of mental organization. This tendency appeared most clearly perhaps in his celebrated chapter on the Stream of Consciousness, and more especially in his doctrine of 'Feelings of Relation' in which we have the first adequate insistence in contemporary writing on this aspect of consciousness.

His conception of the self as a hierarchy made a deep impression, not so much perhaps for its intrinsic novelty, as by reason of the peculiar picturesqueness and force of his description and his pungent sallies at the expense of popular complacencies.

He made much wider use than had any previous English-speaking

psychologist of materials drawn from the pathological side of mental life. This was in part no doubt a reflex of his medical training, in part was due to his intrinsic interest in the unusual. This trait appears very strikingly in the selection of material for his great work on religious experience.

No one thing so radically divided professional opinion of James as his support of the Psychical Research Movement. In England the society began its career under auspices in many respects most favorable and in all respects scientifically respectable, not to say, eminent. But in this country, James for years stood almost alone among men of high intellectual repute, and although he was eminently conservative in his estimate of the results of the work of the society, he nevertheless committed himself to belief in certain mediumistic phenomena in a way which seriously offended many of his professional colleagues. At best, they regarded him as a man whose judgment could not be trusted, at worst as an unwitting backer of quackery and fraud.

His position on the issue was all of a piece with his insistent and never failing protestantism, his passion for fair play, and a just hearing for all sides of every question. He was never afraid to make a mistake if only it were honest, and he started off on a new tack when he found himself in error, as though fallacy were a regular part of the day's work. He had a wholesome contempt, which I have heard him more than once express with characteristic vigor, for the pose of infallibility and essential omniscience assumed by certain distinguished scholars. It seemed to him at once petty and contemptible.

Whether or not we agree with his view of psychic research, it will be a thousand pities if James' attitude in the matter should fail to teach us of the younger generation the high rewards of honest independence and the insidious dangers of moral stultification which lurk in a lethargic or a cowardly intellectual conformity to ruling creed, whether of church or of science.

In this matter, as in the others to which we have referred, we may gratefully look for his most enduring contributions not so much in the specific doctrines which he taught and defended, as in his splendid spirit of eagerness for the truth, his open-minded willingness to find this truth in humble and out of the way places, and his dauntless courage in proclaiming his faith in whatsoever he found worthy.

With few men could an inventory of his accomplishments, however impressive, be so entirely and grotesquely inadequate as with James. It has been a favorite comment upon his writing that in his case the 'style is the man' in an extraordinary measure. And the saying is

true, but what it leaves unsaid is more than what it utters. His personality was fascinating and magnetic to a degree which his writings hardly indicate. This was perhaps in part due to the wonderful play of light in his eyes, in part to the singularly sweet and resonant tones of his voice, to say nothing of the racy picturesqueness of his words. But inwardly no doubt it sprang from the unsounded wells of sympathy in his nature which made every human creature a thing of at least passing interest to him.

As a teacher he was especially sympathetic and stimulating. He overestimated the capacities of many of his students most seriously, or at least, appeared to do so, but this only put them on their mettle to make good his estimate. What he did in this way to encourage his own pupils, he did more widely for struggling talent wherever he chanced to note it. No brochure so obscure, no writer so unknown, but that if James detected any spark of promise, he sent a word of appreciation. How much such encouragement meant to many a young scholar toiling for recognition, no one will ever know. But the sum must be large beyond belief.

William James was that rarest of human beings—a great man who was also simple, kindly, brave and true. His memory will always be with us as an inspiration and a benediction.

JAMES R. ANGELL.

THE UNIVERSITY OF CHICAGO.

VOL. XVIII. No. 2. March, 1911.

THE PSYCHOLOGICAL REVIEW

THE PLACE OF MOVEMENT IN CONSCIOUSNESS[1]

BY W. B. PILLSBURY

All would agree, I think, that the most important advance in psychological theory in recent years is the enhanced value placed upon movement in explaining mental processes. This development has been most marked in America. Here the explanation in terms of movement has been carried out more systematically and to greater extremes. James initiated the movement with his theory of emotion; it was continued and amplified by Dewey and in one form and another has been extended by many of the most vigorous writers among the younger psychologists, not to mention the impetus it undoubtedly gave to the pragmatic movement in philosophy. The new theory has introduced more of practical interest into psychology, has made a break in the development of the theories of consciousness as pure thought, and has increased the tendency toward the concrete and away from the abstract. More recently the motor theory has gone very far. A reader of some of the texts lately published would be inclined to believe that there was nothing in consciousness but movement, and that the presence of sense organs, or of sensory and associatory tracts in the cortex was at the least a mistake on the part of the Creator. All that seems really necessary is the motor cortex with its muscular connections and an occasional path from the sense organs. The marked disparity between these extremes of the theory and the earlier and popular present belief, together

[1] Address of the president, before the American Psychological Association, Minneapolis Meeting, December, 1910.

with the fact that the theory has been little criticized, suggest that it might be profitable to attempt a critical if sympathetic survey of the different formulations of the theory and to compare it with the facts. The theory is so simple and so easy to present that every one is glad to believe it. The only question that any one cares to raise is how much of it will the known facts permit one to accept.

The motor processes have been used by different men and at different times in the explanation of practically every one of the psychological problems. Berkeley gave a motor interpretation to space and has been followed by some writer in every generation. Time, rhythm, and each of the perception qualities has been referred to movement; Bain found movements important in abstract ideas, and the more recent theories have added practically every one of the cognitive processes to the list. If we add to these the affective processes, the clearness of the attentive consciousness, and the subordinate motor processes, there is nothing in mind that has not been explained in terms of movement. The history of the motor theory or even a complete catalogue of the phenomena that have been explained by it would take too long and is doubtless unnecessary in this connection. For purposes of discussion we may divide the explanations that are made through movement or motor processes into three groups,—the explanations of structures, of functions, and of the higher intellectual processes that lie on the border-line between structure and function.

Of the mental qualities the non-sensory components of perceptions and the memory processes have been most frequently ascribed to movement. Space nearly always involves movement for its full appreciation, and it is simplest to say at once that the movements, real or implied, are space or make spatial appreciation possible. Time and rhythm also imply movement of some sort and find ready explanation as motor processes. Memories can be referred to movement by the same sort of reasoning. It has long been recognized that all memories might be in terms of related kinæsthetic imagery. Stricker suggested that kinæsthetic imagery really

was due to revived movements and that it might be merely a reinstatement of old movements. It would follow, then, that all memory was nothing more or less than habit, and that all memory content was neither more nor less than appreciation of present movements revived on some appropriate occasion. No one, so far as I know, has taken the next logical step and suggested that the true quality of sensation is motor. But it is not impossible. All that is necessary is to assume that each sense organ is connected with definite muscles, that these muscles are all excited whenever the sense organ is stimulated and that the colors or tones that we see or hear have their origin in some group of muscles rather than in a sense organ or in the cortex. If we add this extension, all the qualities of consciousness have their origin in the muscles; the brain is merely a switching station through which the real seats of consciousness may be aroused when the appropriate stimulus presents itself.

The most obvious question that arises in this connection is whether the number of possible movements is sufficient to provide the qualities assumed in the theory, and whether the movements are sufficiently accurate and easily enough discriminated to supply all of the qualities. Stumpf long ago pointed out that the area of greatest sensitivity on the retina was within the region for which no movements of adjustment are necessary and the work of Judd and McAllister seems to reinforce this opinion by showing that one is constantly making unappreciated eye-movements that are greater than the smallest space that may be discriminated. It is very difficult to assume that one may make a different sort of movement for everything that can be remembered. The number of movements seems too few and it is too difficult to appreciate the differences between movements. Then again nothing seems to be gained by taking sensations in through the sense organs and out through the muscles, particularly if we accept the current belief that movements themselves are known only through sensations. One has gone a long way round to get sensations again, and when the result is obtained all the problems are still before us. The

circumlocution gives no real gain. We trade colors, sounds, smells and what not for kinæsthetic sensations that are in every respect as hard to deal with as the original.

All this argument is on the assumption that movements are appreciated only through the sensations that come to the cortex from the moving muscles. That was the conclusion that was reached fifteen years ago. Münsterberg, James, Ferrier and others succeeded in convincing the world of science that the old accepted innervation sense had no existence in fact. Even so doughty a scientific warrior as Wundt gave up his position and tacitly accepted his opponent's views. Seldom has a scientific judgment been so quickly and completely reversed in so short a time. But the advocates of the motor theory seem now to attempt to revamp the innervation sense and reinstate it in its essentials, and that without any new proof. Thus we find Münsterberg arguing that the quality of clearness that arises in attention is due to the action of motor cells. A conscious quality is ascribed to the motor area. In several of the other theories we find a statement or implication that the qualities that are ascribed to consciousness as a result of movement have their origin in the cortical motor apparatus rather than in the sensory ends in the body of the muscles.

This change in the point of view is the more surprising since, so far as I have been able to discover, no new facts have been brought to light that favor the view, and the theories themselves attempt no new interpretation of the old facts. The statements are justified only from theoretical needs. Stated in the current form, the theory is obviously very difficult to disprove and is even harder to prove from any definite evidence. The motor centers are not open to observation and cannot be stimulated in any way that would leave the sensory and associatory areas unaffected. It is thus difficult to prove in the ordinary way that the conscious experiences ascribed to the motor areas do or do not arise from them rather than from the other cortical regions or from the more peripheral motor apparatus. Only two crucial experiments suggest themselves. One would be to examine

a case of brain lesion in which the motor areas alone were affected and determine if the qualities in question were still retained. The fatal defect in this test is that if all the motor regions were destroyed there would be no way of communicating with the patient to know what the character of his consciousness might be. The second would be to stimulate the motor regions of a conscious subject and determine what mental states were produced. This test has recently been tried with another object in view. You are doubtless all familiar with the recent experiments of Cushing. You will recall that he performed the plastic flap operation upon two patients under primary anæsthesia and then permitted them to return to consciousness with the central regions of the cortex exposed. These regions were then stimulated electrically and the patients requested to describe their experiences. The localization of the different motor functions was confirmed. The muscles moved easily with the excitation of the appropriate areas. Here is an experiment that to all appearances is strictly adapted to our problem. The motor regions of the cortex are stimulated vigorously with no excitation of sensory or associatory region. On the theories in question one would expect the richest sort of conscious experience. There should have been wonderful appreciation of space relations. All the ideas should have become unusually clear and distinct, and there should have been a rush of recognitory experiences and of feelings of identity. No conscious process would have been entirely unexpected. For this theory the results were completely disappointing. All that either patient felt was a contraction in the muscle, a contraction that was described as exactly like the one produced when the electric stimulus was applied to the muscle itself. And the patients were expert in detecting such movements since part of the earlier treatment had consisted in exciting contractions in these muscles by the induction current. The results are exactly what the strongest opponent of the innervation theory would expect.

The only escape from the conclusion for the exponents of the theories that ascribe conscious qualities to the motor

areas is to argue that the method of stimulation did not call out the same sort of response that would be called out by the ordinary stimulation through sensory and association areas. While one must admit that this argument has some force, to accept it removes the motor theory from all possibility of direct proof. It would then say in effect that the only sort of excitation of the motor areas that gives rise to consciousness is the one that originates in the sensory and associatory regions, and that at once prepares the way for the answer that it is these other areas of the cortex that supply the conscious qualities. All experimental results, then, since the case for the motor regions as the seat of consciousness was reopened, and these most striking neurological experiments of Cushing confirm the judgment that was passed on the case twenty years ago. The evidence all favors the statement that movements are known only through the excitation of the sense ends in the muscles themselves and that these arouse consciousness only as they excite the kinæsthetic areas in the cortex. The large mass of theory that has developed in recent years on the assumption that the motor regions contribute to consciousness flies directly in the face of the evidence. So far as it is possible to know the facts the only structural contributions of movement to consciousness are the kinæsthetic sensations. The attempt to take the motor contributions of consciousness back into the motor cortex and away from the muscles has no warrant in fact. Its only advantage is that it puts the theory in a form that is difficult to disprove, even if it is impossible to prove. Granted that movements are known only by the kinæsthetic sensations, to translate all conscious qualities into motor terms merely transforms all other sorts of sensation into the one sense, and that a sense relatively poor in qualities.

To assert that all memories and all the elements of perception that do not arise immediately from sense are motor in character is to assert only that these are kinæsthetic sensations. And this does not help in the solution of the problem nor does it square with direct observation. To reinstate all sensory experience in motor terms merely reduces all

memories to one sense. To be sure an occasional man does translate all experiences into kinæsthetic terms for the purpose of recall,—is of the kinæsthetic memory type, but he cannot be made a law for all. Direct experiment or observation shows that most men have other qualities in memory and also as supplements of the raw sense materials in perception. Were it not so it would seem a great waste of cortex. There is no reason why the relatively small kinæsthetic area should be the only one used in recall. The only justification for the assertion that motor processes are alone valuable for memory and perception is that both memory and perception deal primarily with meanings rather than with content, but as we shall see meanings are not necessarily motor.

The first of the more dynamic effects ascribed to movement is the selection of one from the possible mental states that might present themselves at any moment. This selection may be of sensations, of ideas or of movements. The fact and its conditions are the same in any case, and the motor explanation has been or may be applied in one instance as well as in another. When ascribed to sensation it receives the name of attention, when applied to ideas it is reason, when applied to the selection of acts it is approximately identical with will. In any one of these cases not the intensity of the original sensation, nor the strength of the particular association or habit determines the character or course of the conscious process, but it is determined by something outside of the immediate experience. Certain representatives of the motor theory insist that the selecting agency in all of these experiences is the state of preparedness of the motor areas that are connected with the receiving sensory region. If two objects present themselves to the sense organs at the same time it is asserted that that one will actually be perceived which finds the sensory apparatus connected with a motor mechanism already active or in a state of preparedness for action. Similarly if an idea is equally closely connected with two other ideas, the one connected with a motor mechanism, active or partly active at the moment, would be recalled. Logically, when a stimulus had become

the initial member in two habits of about equal strength, the choice between the habitual responses would depend upon which set of muscles was at the moment more prepared to act. Selection, on this theory, depends upon the openness of the way out, not upon the force exerted by the stimuli that are coming in.

The difficulties for this explanation become obvious the moment we ask why the motor tracts are in different degrees of preparedness. If one stops with the theory as stated, all selection and, hence, all consciousness is a matter of chance. If the explanation be pushed back a step farther, the only explanation for the momentary openness of one path of motor discharge must be found in some other sensory stimulus, or in sensory stimuli that have been working just before and have left the motor tract in readiness for response. Thus, if I stand with glass in my hand, hesitant whether to drink or to replace the glass upon the desk, it might be said that the decision was in terms of the readiness of one or the other group of muscles. But this tells nothing of the real occasion for the decision or of its important antecedents. That can be answered only if one can say why the one set of motor organs is active, rather than the other. To answer this leads at once back to sensory excitations, old habits, and present circumstances which suggest the old habits. If the throat is giving strong sensations of thirst, the drinking response will be more active; if embarrassment is felt and the presence of the audience suddenly becomes oppressively prominent, the downward response will be the readier. In any case, one cannot understand the situation or explain choice, if one stops with the statement that the motor tracts are active in one way rather than in another. Selection in attention offers the same difficulties, and the motor explanation has the same limitations. Selection, if adequate, must depend upon the knowledge of the environment,—upon the attitude, sensory as well as motor. It may be said that the considerations act first upon the motor tracts and then back upon the sensory and associatory regions, but this is not at all assured. It is altogether probable that the secondary, or preceding sensory determinants, are coör-

dinated in association tracts, rather than first finding their expression in the motor regions. Certainly no one can be sure that the point of interaction and coördination is not in the associatory areas, rather than in the motor regions. In short, the explanation of selection, in all of the forms that the motor theory presents, either is not motor or it is not an explanation. Selection seems to be no more exclusively dependent upon the motor parts of the cortex than are the qualities of consciousness. The important elements in each are the sensory stimuli and their interaction with the results of earlier experiences; the motor areas are altogether subordinate if they play any part at all.

The more general phases of the motor theory and the explanations of the higher mental functions seem to have grown out of very much the same set of considerations. The great advance that has been made in discussing psychology in recent years has consisted in attempting to ascertain the value of the different parts of consciousness. Consciousness as the sum total of sensations or of experiences is a complete chaos. There is no reason why one part should be more important than any other. No one element should stand out prominently over the others. The actual consciousness on the contrary is always unified, is directed to certain ends and organized with the emphasis upon definite parts. The motor theory looks to action at every point as the end and the means of the organization. Action is the usual end of all thinking, just as movement is the ordinary outcome of all action of the nervous system. Not only that, but most consciousness deals not with qualities or particulars of any sort but with events, with groups of qualities, and these groupings, the motor theory asserts, are made by the actions. A thing is not primarily an agglomeration of sensations; it is an occasion for action, and the sort of action goes far to determine the character of the thing. One does not reinstate a group of qualities in remembering an event, one remembers that the event happened. And the thing that happened takes its importance and its character from the actions that it called out or that it might have called out. A memory, then, is

nothing more than some way of representing these earlier actions, and what more suitable than a reinstatement of some element of the old movement. Any form of thought may be referred to movement in the same way. In short, consciousness is like a funnel. The end that receives impressions is very large while the end that lets their effects out into the world through movement is relatively small. The suggestion of the motor theorist is that it is easier to understand the small end than the large; that acts are the natural systematizers of the original chaos of consciousness, and that we should look to them for the organized materials.

All of this must I think be accepted as a general principle. Action is the natural end of thought. Thinking, too, is organized, not chaotic, and organized about acts. Most of thought is only schematically representative of the events and things; the events and things are not represented in detail. The only question is whether action is the only element in the organizing process, and whether the representatives of all things not explicitly reproduced in consciousness must necessarily be motor in character. In general it seems that the motor theorist has been beguiled by the ease of explanation in very much the same way as the older sensationalist. If all sensory processes do lead over into movement and the sensations themselves are too rich in multiplicity and too little organized to explain the well ordered course of thinking, nothing is easier than to say that thought is movement and that all the higher functions, all the representative processes can be explained by the movements that are evoked. This is perfectly simple, and near enough the fact to be plausible. The difficulties begin to present themselves, however, when one asks how the movements can have representative value when taken out of the setting in which they originally presented themselves. How, for example, can the movement that I made when I first thought of a triangle have a greater representative value than an image of a triangle. How in general can any movement be any more representative of anything beside itself than any other particular mental process. It is just a bare movement appre-

ciated through kinæsthetic imagery, as a visual image is a bare image. Neither should have any representative value as it stands. To understand how one can take on meaning is just as difficult as to understand how the other can represent things beyond itself. Certainly a movement has no representative value when it is made for the first time. What changes when the movement does take on meaning is not the movement itself but something added to the movement, and this something might be added to any sensation just as well as to the movement.

The motor theory breaks down in the same way when at all critically examined in its applications to the other functional problems, as well as to meaning. Take the related but slightly simpler problem of recognition. On the motor theory I am supposed to recognize a friend because I make the same movements now that I made when I last saw the same friend. I presume this means that I must have a specifically different smile for each friend, or a characteristic form of salute. Granted that this was the case, although it would require a very large repertoire of smiles, the problem would at once arise how I recognize these smiles as different. That is just as much a problem when translated into movement as when stated in visual terms. This becomes particularly evident when one considers how little our different movements are discriminated in every-day life. One makes movements every moment or two that pass entirely unnoticed. I have corrected my balance hundreds of times during the present half hour without once being conscious of it. The different movements are not at all easily discriminated, as is apparent to any one learning a game of skill. In golf, one thinks the same movement has been made as when one got a good drive, at the last tee, but the ball goes far out of course or does not move. When appreciated, movements are not appreciated for themselves but for their meanings. In this they are like any other sensation. The only way that they are different in this respect from other sensory qualities is that they seem to have rather less distinctive character, to be more completely dependent upon translation into other terms and upon their

connections for their comprehension and interpretation, — for recognition and for the discrimination of one movement from another. Far from being independently appreciated, and a suitable means for the discrimination and reference of other mental qualities, movements are, if possible, more dependent upon outside factors for their appreciation than visual or auditory qualities. They have no meaning of their own as independent entities; they are not easily recognized; they cannot always be identified. They possess none of the almost magical virtues for themselves that they are assumed in the motor theory to be able to impart to the other sensory qualities and mental states.

To understand how any thing or process is recognized or why any image has meaning, one must look not to structure of any sort, but to function, not to any element of consciousness but to the connections of elements in consciousness. The assumption of certain of the motor theorists that motor explanation of the extreme sort and functionalism are identical seems far from self-evident. To be sure there are many things that we think without explicit mental imagery, and the imagery that we have when we think them is indifferent to the things that are thought. This function of the image or of mental representation in general offers one of the most important problems for psychology. It is the problem of use in the broad sense as opposed to the problem of elements or structures. The structuralist has occasionally tried to solve it by transforming function into some peculiar structure different from the ordinarily accepted elements of consciousness. This it must be admitted does not solve or even face the problem in its essence. But the solution of the motor men is on exactly the same level. They would transform function into movement as the sensationalists have at times attempted to transform it into sensation or related content. As is evident from the earlier discussion, movement is as much a structure as sensation, and is as little capable of solving problems of function. The motor theory of itself is not a functional theory, — it is merely a new way of palming off structure as function.

The only other ground on which a motor theory could be called functional or exclusively functional is on the assumption that movement is the end of life and that all of man's accomplishments are of a physical character. On that assumption man's only function is to move, the only outcome of thought is action. But this theory is too superficial to be intended by the motor man. To say that all functions are to be explained by movements would be meaningless if function meant nothing more than movement. All agree that, however important action may be in the final outcome, action is only possible or at least adequate because of a large amount of antecedent thought. Furthermore a large part of the conscious life is important for itself; the result in action is long delayed if present at all, and what seems important at the moment is the intellectual satisfaction, or some other contemplative passive accomplishment. A functional psychology in the larger sense must recognize functions of this sort as well as the products of immediate action. A complete functional psychology must treat mental accomplishments of any sort so far as they are not definitely embodied in structures, or so far as the structures are not prominent nor essential to the accomplishment. Functional problems in whole or in part concern the nature and conditions of selection, the nature of meaning and belief, the reasoning processes in general and much of memory and imagination. It must be admitted with the motor men that movement is nearly always involved in these operations either as antecedent or consequent or both; the only point in controversy is whether the movement is the essential condition of the functions or whether the functions are the natural conditions of the movement. As has been shown above, when carefully examined and tested the explanation in terms of movement does not put us any farther ahead. It is necessary to turn back to examine the antecedents of the movement before one can understand the movement. This is granted by many of the men who uphold the theory that I have been criticizing. They insist however that to ask how the movement originates or to question how it gives value to the con-

scious processes that develop from it clouds the issue. I must confess that I cannot follow them. To admit that a movement is determined by the attitude or the organization, and then to deny that these antecedents have any real value in the explanation seems inconsistent or at the best arbitrary.

When reduced to the last analysis, the difference between the motor theory of these higher processes or functions, and what might be regarded as the true functional theory is really slight. The former would insist that all of the essentials in the explanation are to be found in the movements that are made at the time, while the other finds an explanation in the organization and interconnection of experiences that precede movement. There is no time to go into the nature of this organization now, and complete exposition is the more unnecessary as there is fairly close agreement as to the more fundamental principles. In short there is difference of opinion only as to whether organization or attitude is the result of movement, or whether movement of any rational, intelligent sort is the outcome of organization. The difference is the less important since all would agree that among the important elements in the development of any organization or attitude are the earlier movements that have been made in similar connections, and that movement contributes many important qualities to the consciousness of the functions that do not lead immediately to movement. One believes that movement is the only cause of the organization, the other that it is one important cause of that organization.

One other more general consideration has been adduced in favor of making movement the determining element in mind. It is argued from the fact that movement is the end of all thought, that the movement must induce the thought from which it originated. When for example a period of reflection culminates in a stroke at billiards, the stroke must be regarded as the cause of the thought. One must grant that the thinking would not have gone on as it did if the movement had not been contemplated, but it is a matter of temperament whether one regards the movement as the outcome of

the desire to win and the other antecedent thoughts and habits or of the thoughts as looking forward to the act and that to the outcome of the game. Any fact, mental or physical, can be explained in terms of all the antecedent events in the relevant series or in terms of all the succeeding events in the series, but one gets a different explanation as one looks forward or back for it. One can only insist that if one explains the mental process by the stroke that it starts, one should also be prepared to hold that the movement of the ball induces the stroke of the cue, and that, in turn, the muscular contractions, just as the muscular contractions induce the mental states. There is no hope of settling the rival claims of teleology and causation; all that can be done is to indicate that the explanation under discussion is teleological, not causal.

In this criticism of the motor theory I have refrained for the most part from ascribing the phases of the theory to different authors. This was partly from desire to avoid personal controversy and partly because it was frequently difficult to pin a theory to any one man. Several times when confirming a reference to an author who had been credited with a theory I found that his writings gave little warrant for the view assigned to him. I am quite free to admit that in several instances I may have been fighting wind-mills, but at the risk of belittling my audience I have tried to make clear what wind-mills I was fighting. I believe that each of the positions that I have contested is held to-day by some reputable psychologist, and I presume that there is no psychologist of standing who does not somewhere hold some one of them.

It is not the intention of this address to undervalue the importance of the motor theory or even to refute it in its essentials. As was said in the beginning, it is one of the most vital movements in modern psychology. It does seem, however, that just at present it is being carried beyond the point where it can be justified by the facts, and much as one may regret the necessity it is well to recognize the limits beyond which it cannot be extended. If we summarize, the motor theory is right in emphasizing the part played by kinæsthetic

qualities in every domain. They constitute an important part of every perception process; they color memory, and give tone to all of the intellectual operations. On the active side they constitute the goal of all thought, they have played an enormously important part in the organization of experiences into systems of knowledge, and so in making thought possible. Perhaps more important than all, the motor theory has insisted that no mental process can be understood in and of itself. It has rendered impossible the older form of explanation in which mental states were regarded as entities that were independent of all other mental activities present as well as past. Interrelation, context, attitude are now made the basis of all forms of mental explanation rather than elements or entities. The advance has been a noteworthy one.

On the other hand the motor theory certainly claims too much. Not all qualities of perception or memory are of motor origin. And what is of motor origin cannot be ascribed to the motor cortex directly, but only indirectly through the kinæsthetic sensations. Also no meaning can be given to the assertion that selection of conscious processes is in terms of movement. Rather one must look to the results of organized early experience for an explanation at once of the direction of mental states and of the movements themselves. The attitude of the moment determines movement, not movement the character of the attitude. Again the higher mental functions, although they involve movement, cannot be explained in terms of movement alone. Movements in and of themselves have no meaning, are not immediately recognized nor understood. These functions require just as much explanation when they attach to movements as to any mental process. Movements, too, acquire meaning, are recognized or judged only as they may be referred to other mental states and finally to a systematized knowledge. To identify the motor theory with a functional psychology is to destroy the essence of the functional view of life and reduce functions to new and inadequate structures. Finally, movement cannot be regarded as the cause of any of the antecedent conscious states. While the teleological view may have

some standing in other disciplines, it has no place in science; at the least it should be plainly labelled when introduced into a scientific discussion. It should be added that even after these inadmissible phases of the motor theory have been lopped off, the cardinal principles have not been touched. It still marks a great advance over the theories that preceded it.

If the earlier theory placed too much emphasis upon the sensory regions of the cortex and sensational qualities, this theory too much neglects them. Again it must be emphasized that all action is sensori-motor, that movement alone will not explain consciousness any more than will sensation alone. Movement grows out of sensation just as truly and inevitably as sensation gives rise to movement. Consciousness is made up of sensation understood with reference to movement, and of movement in the light of and under the control of sensation. More important than either sensation by itself or movement by itself is the fact that consciousness is always an organized system. This system is constantly growing with and by use, and to it every experience contributes. It is constantly receiving new contributions from sensory elements and each stage in its development is tested by action. That movements and thought both are ordinarily controlled by the system is the element of truth in the statement that meanings and related functions are motor. It is the system that both controls movement and gives meaning to the new experience. To assert that it is movement which gives meaning is but to substitute the result for the cause. Movement is one of the most frequent outcomes of organized experience and may be regarded as the sign or symbol of its presence. A complete explanation of any phase of consciousness can be neither in terms of sensation nor in terms of movement exclusively, but must include both. Both must be seen in the completeness of their reaction one upon the other, and of one sensori-motor circuit upon others. Above all there must be constant reference to the ever growing system of knowledge that develops out of sensation, that is tested by movement and which alone gives meaning to sensation and rational direction to movement.

EXPERIMENTAL STUDIES OF RHYTHM AND TIME.[1]

BY J. E. WALLACE WALLIN, PH.D.

Clinical Psychologist to The New Jersey State Village for Epileptics.

I. QUALITATIVE LIMENS OR GRADES OF RHYTHM, AND THE DIFFERENCE LIMEN IN THE PERCEPTION OF TIME

In an earlier investigation of speech rhythm,[2] it was found that there were various qualities or degrees of speech rhythm, and that the advocate of the 'time theory' was incorrect in so far as he insisted on absolute periodicity as the *sine qua non* of rhythm. For in measuring phonograph records of recited verse, it was found that the listener was able to 'easily coördinate' the intervals or measures when they varied as much as 7 per cent. A variation of 12 per cent. rendered the rhythm vague, and a variation of 15 per cent. made it 'hard to rhythmize the impressions.' The rhythm was 'destroyed' when the variation reached one-third of a second, 'disturbed' when it reached one-fifth second, and 'well preserved' when it amounted to only one-tenth second. These results suggested the desirability of making a more

[1] The following research was carried out in the Princeton Laboratory in March and April, 1904, and a brief report of the same was presented at the Boston meeting of the American Psychological Association in 1905. Owing to various circumstances, chiefly the heavy teaching duties incident to normal school work, the publication of the complete data has been unduly delayed. It is regrettable also that my time is now so heavily taxed in the pursuit of other problems, that it will be possible to do little beyond writing a brief analysis of results and pointing out the more obvious deductions which follow. Unfortunately the same must apply to two or three articles which will follow in this series as soon as they can be prepared for publication. It is the intention, however, to reproduce the important original data in fairly complete tables, in order to make the material available for further study by future students of the problems of rhythm and time. Should time permit it is also intended to summarize at some future date the chief results in a final synthetical article, in which larger comparisons with the results of other investigators and wider conclusions will appear. Since, therefore, the articles in immediate contemplation will be analytical in character, the statements will be largely limited to a mere account of methods, findings and results.

[2] Wallin, 'Researches on the Rhythm of Speech,' Studies from the Yale Psychological Laboratory, IX., 69f, 128, 135, 1901.

careful determination of the amount of irregularity that can be introduced into rhythmical intervals without affecting, impairing or disrupting the rhythmical impression. The problem of the present investigation may, then, be stated as follows: how much may the time of rhythmical intervals be varied without perceptibly affecting the rhythm? And how many grades of rhythm can be differentiated? That is, how many steps or limens of rhythm can be produced by a constant increase of the time differences between the intervals by minimal steps? Is there a psychophysical constant of rhythmical variations?

The rhythmical material used to solve this problem consisted of the tones produced by the vibrations of a 500 D.V. fork (operated electrically) and amplified by a Helmholtz resonator (U_4). To break up the continuous tone made by the fork into alternating series of tones and silent intervals, a notched disc was constructed of heavy stiff non-warping paper board and made to revolve between the fork and the resonator in such a way that the notches would pass before the hole in the resonator. This disc was rotated by a Ludwig-Baltzar kymograph. To determine the regularity of the revolutions a 100 D.V. fork was made to trace a time line on a smoked paper pasted to the disc. Twenty-one of these time lines, measured from records made on two occasions, gave an absolute variation (M.V.) for each rhythmical measure or interval (60° of the disc) of .017 sec., and a relative variation of .0158 sec. (M.V./A. $A = 1.075$ sec.). The M.V. for each degree was accordingly .00028 sec.; and the M.V./A, .016 sec. Changes in the speed of rotation or irregularities were remarked only a few times by three subjects (D., W., Wa.), so that any slight variation in the mechanical regularity of the kymograph was probably negligible.

The disc was twelve inches in diameter. The grooves or notches were cut radially at the periphery. The shallow ones, giving the weak tones, were three eighths of an inch deep, while the deep ones, giving the loud tones, were six eighths of an inch deep. The depth of the latter was approximately the depth of the opening in the resonator, so that they

gave the maximum intensity of sound furnished by the resonant tone. The relation of the intensity of the loud and weak tone was thus in the ratio of 2:1. There were six of these grooves in the disc, three deep and three shallow, the distance from the beginnings of the grooves being 60°. Each groove was 10° long, so that one sixth of the interval was audible. Accordingly one revolution of the disc furnished three loud and three soft tones, or three rhythmical measures, as indicated by the following scheme, in which the large dots indicate the loud tones: $\overset{1}{\bullet}\ .\ \overset{2}{\bullet}\ .\ \overset{3}{\bullet}\ .$
By reversing the disc on the kymograph, the iambus pattern (⏑ —) could be obtained: $\overset{1}{:}\ \bullet\ \overset{2}{:}\ \bullet\ \overset{3}{:}\ \bullet$

The fork tones were transmitted to the subjects in an adjoining room by means of a rubber tube. One end of this was fitted over the resonator opening and the other end terminated in an improvised cardboard megaphone which was held close to the left ear by the subjects. The tones were easily audible. No sounds from the workings of the apparatus reached the listeners.

Two speeds or tempos were used. With one the rhythmical measure or interval length was 1.075 sec., and the duration of the tones .179 sec.; and with the other, 0.57 sec., and .095 sec., respectively. They were approximately twice as long in the one case as in the other.

Two methods of giving the tones were employed. In the 'continuous' method the subjects were allowed to listen to 18 successive tones or nine rhythmical intervals as the disc made three uninterrupted revolutions. In the 'discontinuous' method the tube was closed by squeezing together firmly the prongs of a pair of wing dividers during every alternate rotation of the disc. Therefore after the subject had heard six consecutive sounds, a period of silence intervened. This was succeeded by another period of sounds, then a period of silence, etc. Three subjects (S., D., Wa.), however, remarked that they got a faint impression of the sounds while the tube was closed. Any disturbance caused by this factor is probably negligible, so far as the general averages are concerned.

The regular variation in one of the interval lengths was effected by means of a 'variable' tone. One of the shallow grooves was made wider than the others, 28°. To the disc at this point was attached a sliding piece of zinc, containing a groove of standard width, namely, 10°. The moving of this slide shifted the sound, and thus made one interval shorter while the corresponding interval on the other side of the beat was made longer. The movement of the disc was made by hand, the amount being regulated by a degree scale drawn on the disc. The variable was always the last tone of each rotation in the trochee pattern (the sixth). The shifting followed the method of minimal change,[1] the amount of each change being usually one degree while the interval lengths were about equal (infrequently steps of one half degree were employed), and two degrees as the difference became pronounced. The subjects knew the direction of the change, but not the size of the steps. Each test always started with the tones objectively periodic. The differences were then gradually increased until the rhythm was considered destroyed (receding or ascending direction). Hereupon the movement was reversed (approaching or descending direction), the differences being gradually diminished until a perfect rhythm was again reached. The approaching series was usually started on a different reading than the last of the receding series.

The disc was always allowed to make three fourths of a revolution before the tube was opened (*i. e.*, before the subject was permitted to listen to the tones), in order to allow the kymograph to attain the right speed. The first sound released in the trochee pattern (— ⏑) was the first loud one after the variable; and in the iambic, the first weak one. The slide was displaced in one direction only, so that the interval vetween the fifth and sixth tones was lenghtened, and between the sixth and first shortened in the trochee pattern; and shortened between the fourth and fifth, and lengthened between the fifth and sixth tones in the iambic

[1] Woodrow's application of a similar method to the study of the rhythm and interval indifference points is accordingly more recent: 'A Quantitative Study of Rhythm,' Columbia University Contributions to Philosophy and Psychology, XVIII.

(the variable in this case being the fifth tone, as the loud tone came last in the measure).

The lengthening of one interval produced a compensatory shortening of the adjacent interval, except in the discontinuous method, trochee pattern, where no shortening occurred, because the sixth tone was followed by a period of silence, as already explained.

It is interesting to observe that the lengthened interval was noticed far more frequently than the shortened, as shown in detail in Table XIII. There was not a single subject who did not perceive that an interval had been lengthened, but the reverse was not true. Few of those who perceived the shortening did so at the beginning of the series; most of them noticed it only after the difference had become pronounced, or after it had been suggested by the experimenter. There was often a curious see-sawing between the two, in some cases probably due to the shifting of the attention from the one to the other. But as a rule as the intervals approached equality, the shorter interval tended to disappear. So far as the trochee pattern is concerned the explanation seems to be that the lengthened interval preceded the shortened (law of primacy), but this is not the case for the iambic, where the result is about the same.

It is possible that the sound of the variable, owing to the use of the zinc slide, differed somewhat from the other weak sounds. Two (M., McD.) remarked that the last beat 'clicked off' differently, or that it was pinched off or drawn out. A number reported slight irregularities with regard to the tones, such as that one of the loud tones seemed shorter once (F.), or longer (D., Wa.), or louder (D.); or a soft one seemed shorter (McD.) or longer (M.); or all the tones seemed fainter (C.), or some seemed to drag or stretch out more (Wh., W.). These were sporadic observations, and probably were mainly subjective effects. It is possible that on a few occasions the tones were all objectively fainter, owing to the occasional running down of the cell. On the whole, any objective difference between the variable tone and the two other soft ones was probably too small to affect the results adversely.

One sitting of about one hour was required to finish an experiment with each method for a given pattern and speed, expect that in the case of the shorter speed (0.57 sec.) both trochaic and iambic patterns were finished in one sitting. Each subject thus gave four sittings to the investigation. It was impossible to make more than one determination in each direction (ascending and descending series), although some of the steps were repeated while the subjects were asked to pay special attention to the rhythm or the time;[1] nor was it feasible to take the subjects at the same hour of the day. Five subjects (C., K., McD., M., S.) found it rather difficult to attend, on one or two occasions, owing to having slept poorly, being fatigued, depressed or hungry, or having just exercised; but with very few exceptions the observers testified that there was no difficulty in giving full attention to the experiment. The observers, besides Professor Warren, were students who had taken an introductory course in experimental psychology. The instructions given were uniformly: to sit erect, hold the megaphone close to the ear, to attend well, avoid haste and give disinterested responses. Uniform introspections were likewise called for, concerning the following items: the condition of the subject during the sitting, the regularity of the rotation, the preferred tempo and method, the fact as to whether the lengthening or shortening of the intervals was more apparent, the degree of the feeling of rhythm actually aroused by the tones, the nature of the processes which usually give the keenest perception of rhythm, the mood produced by the tempos or patterns, the differences produced by changing the variable upon the time and the rhythm, etc. To rule out the factor of time order, some subjects began with one method or pattern, and others with another, as shown in detail in the tables. But all the tests for the faster tempo, 0.57 sec., were begun after the experiments with the slower tempo were finished.

It is well to emphasize the fact that the attempt has been

[1] Occasionally steps were repeated three or four times without any such instructions. In such cases the results in the tables represent the average of three or four determinations.

made in this investigation to use tones which offer an *objective* rhythm; that is, regular recurrences of loud and soft tones—genuine objectively rhythmical groups. Not infrequently experiements on rhythm are based purely on periodicities which have little in common with rhythmical groups on the objective side. Whatever rhythm is felt is subjective. In this experiment, where objective rhythms were used, repeated enquiries were made as to whether or not the subjects actually experienced any rhythmical feeling. The results are embodied in Table XIV. In the 'preliminaries' (with the slower tempo only) the subject was merely told to listen in the megaphone. After a few rotations of the disc he was asked to describe his experience. This was repeated once or twice. A record was made as to whether the subject noticed that strong and soft tones were used, whether a rhythmical feeling was produced, and whether the tones seemed to come regularly. In later tests the subjects were asked to mention experiences or instruments which produce a rhythmical feeling of *like* intensity, and one of stronger intensity, the experiences which give them the most intense rhythmical impressions, and whether or not they found themselves using movements of any kind when listening to these tones.

An examination of this table will reveal a number of facts to which attention may be directed at this time. It appears from the preliminary introspections, made at the beginning of the first sitting, that all the subjects, except possibly a couple, experienced a genuine feeling of rhythm. This feeling apparently was no stronger, at the beginning, than the feeling of rhythm experienced when listening to a bell, an organ, a clock or metronome. It must be observed, however, that the feeling of rhythm received from the 0.57 sec. tempo was better than the rhythm from the slower rate. All except one of the subjects preferred the faster rate, eight because it gave a better feeling of rhythm, six because it made the tones shorter, sharper or more distinct, four because it made them sound louder, three because the changes were perceived easier, and two because the tones were easier to

follow.[1] It is probable therefore that practically all subjects (McD.'s rhythm sense seems undeveloped) experienced a rhythmical feeling, which grew stronger as the tests proceeded, even with the slower rate, and which were more intense, particularly with the faster rate, than the rhythmical impression produced by the regular tones of bells or metronomes, but in no case as strong as the beating of a drum. The keenest appreciation of rhythm, according to these reports, comes from dancing, band and orchestra music and drum taps. The additional fact, that no extensive use was made of movements in order to aid the rhythmical feeling, also indicates that the tones were adequate to produce rhythmical experiences.

We may conveniently consider the results in more detail under two headings.

I. The Differentiation of Rhythm into Qualitative Grades

On the basis of the results we are able to recognize five grades or qualities of rhythm in relation to the amount of irregularity introduced between the beats of the rhythmical series. These grades may be designated 'excellent,' 'good' (but somewhat shaky), 'medium' or 'fairly good,' 'poor' (or very jerky) and 'disrupted' rhythm. The average amount of time displacement or irregularity in terms of per cent. of the interval length for these five grades amounts, respectively, to the following: 6.36 per cent. (rhythm of grade I.), 8.53, 12, 14.5 and 17.8 per cent. (grade V.). These per cents. are based on the general averages for both speeds, patterns and methods (Table VI.). In absolute units of time the corresponding figures are: .0526, .0734, .0991, .1182 and .1488 sec.

[1]The *moods* produced or the *effects* of the two tempos upon the subjects were as follows:

0.57 sec.: Made me rather impatient (F.), agreeable mood (S.), spirited, slightly too fast (D.), lively impression (McD.), stirring (W.), a little too fast (M.), soothing, joyful (Wh.), tendency to beat, makes the rhythm pervade your body (K.), more restful, a little faster better (Wa.).

1.075 sec.: Soothing, gives time to think (F.), too slow (S.), soothing, soporific (D.), rather too slow (McC.), rather depressing, too slow (W.), soporific, too slow (Wh.) rather slow, soothing or indifferent (K.), rather too slow (Wa.).

These results have an important bearing upon the controversy between the so-called 'time' and 'accent' theories of rhythm. It is apparent that the time theory, which has been defended by Lanier, Gurney, Hauptmann, Westphal, Lobe, Herbart, Lotze, Schopenhauer, Bolton and Edwards,[1] is not strictly true. Absolutely periodic or regular occurrences are not essential to the appreciation of rhythm, although absolute regularity improves the quality of the rhythmic impression. To engender a feeling of rhythm always requires a certain amount of *periodicity;* but the margin of irregularity that may obtain is quite considerable, as seen above.

It is noteworthy that all of the subjects were able to group the rhythmical qualities into these five grades, rather than into merely three. It has been assumed that unaided psychological discrimination is capable of accurately making only a three-fold differentiation: equal, greater and less. It did sometimes happen, however, that a subject would skip a class; thus, Wa., though infrequently, tended to skip from the second to the fourth grade.

There was no means of determining the reliability of the subjects' judgments except the harmonious working out of the results and their own opinions. The subjects were asked at various times to state whether they felt certain of the grading, or whether the judgments were mere guesses. Two said they were very certain (S. and Wa.; except when near the point of equality for S., and except that it was sometimes difficult for Wa. to tell where he was); four that they were certain (C., K., S., Wh.), and two grew more certain as the experiment proceeded (D. and K.). C. was not sure in one particular test (1.075, continuous, — ⌣). It is believed that the degree of certainty is sufficient in practically all cases to render the results quite reliable.

The subjects were also asked, particularly during the last sitting (with the .057 sec. temp), to state which steps in the gradation were the easiest, and which the hardest, to determine. Three considered that the first grade (excellent,

[1] Wallin, *op. cit.*, 128.

I.) was the easiest (W., M., K.), two the fifth grade (F., W.), two from the fifth to the fourth, approaching direction (S., Wh.), and one each the fourth (D.), fourth and fifth (S.), and from the first to the second, ascending or receding direction (Wh.). Two each considered the following the most difficult judgments to pronounce: the third grade (F., W.), the second (D., K.), four and five (McD., Wa., ascending), and two to one, descending (S., Wh.); and one each the following: fourth (W.), fifth (M.), from one to two (S.), from two to three (Wa.), and from three to four (K.). It appears therefore that there is little agreement in this matter. Possibly the conclusion may be justified that, altogether, the first and fifth grades are differentiated most easily, and the third and fourth with the greatest difficulty.

Another point on which information was elicited was as to whether the grading was a matter of judgment or of feeling. Of the six subjects who reported upon this matter, four felt that they based their opinions upon feeling or a rhythmical sense (C., F., Wa.), two describing the feeling as a slight shock (S., Wa.), and two stating there was no sensation of pain present (C., D.). One did not know (D.), and one considered that it was a matter of judgment. Further enquiry was made as to whether it was easier to apprehend the time differences or the differences in the quality of the rhythm. The opinions of several subjects varied more or less in different tests, but seven reported at one time that the difficulty was about the same (C., D., K., M., W., Wa., Wh.); the two series, the time and the rhythm, ran parallel, the rhythm depending on the time (Wa.). Five considered that the rhythm problem was easier (C., D., F., S., Wa.), except for steps three, four and five for S., and except for the matter of arranging the rhythm into various classes for Wa. Only three considered the time problem easier (F., M. and K.; S. did so also, but only for the approaching direction). On the whole, then, it may be said that it was slightly easier to differentiate between grades of rhythmical qualities than to notice differences in time. These results taken together seem to point to the fact that time and rhythm do not rest

upon precisely the same basis. Rhythm is perhaps less a matter of judgment than of feeling or a rhythm sense. To occasion a genuine rhythmical reaction a movement in time must arouse those sensory processes, and motor reponses or physiological reactions which lie at the basis of the feeling of rhythm. Without an active functioning of the physiological or neural substrate, the rhythmical consciousness would consist of only a certain awareness of a quasi-rhythmical movement in time. The ideational arousal of certain revived processes—the images of the sensory experiences which underlie the rhythmical experience—would no doubt suffice to produce a semblance of rhythm, but it would lack the vividness and intensity which usually go with genuine first-hand experiences and which ordinarily are lacking in revived experiences.

Owing to the fact that the subjects were regularly making reports on the observed changes in both the time and the rhythm (especially at the beginning and end of the series), they were asked occasionally to disregard the time and attend exclusively to the rhythmical impression. The general results for the 'attention series' (which ran parallel with the regular series) are embodied in Tables XI. and XII. It is noticeable that the threshold values were smaller in sixteen cases when the attention was centered on the rhythm, and larger in only four cases. Attending to the grades permits of less variation in the intervals. The differences, however, in terms of absolute time, are merely nominal. If we compare the general averages of Table VI. with the similar averages of Table XI. (they are not precisely comparable because in the attention series the results are often available for one direction only, /\ or \/), we find that the gains for grades II. to V. amount to the following: .0012, .0011, .0080 and .0132 sec. But however small the improvement, if we concede that attending to emotional processes tends to dissipate them, then the results are in harmony with the view that rhythmical experiences are classifiable with sensory processes, and can therefore be rendered clearer by being attentively regarded. It follows as a consequence that the way to get pleasure from

rhythm is to attend to the impressions rather than the agreeableness of the experience—assuming, of course, that the movement is objectively regular.

It will be observed that the extremes found in this investigation for simple tones, 6.36 to 17.8 per cent., correspond fairly closely with those found in the earlier investigation in speech rhythm, 7 to 15 per cent. That is probably more than a coincidence. While the amount of variation needed to produce the various grades will differ, no doubt, to some extent with the character of the material (tones, speech, music, etc.) and the nature of the variation (here only one measure out of three was irregular), yet the ratios will probably be about the same. Such would be the presumption if there is a law regulating these changes—to this we will revert presently. It must be remembered, however, that in the case of the earlier speech rhythm investigation the initial category signified measures that could be 'easily coördinated' the last category those that were found 'hard to rhythmize' (instead of those in which the rhythm had been disrupted), and that no attempt was made in the speech investigation to determine the greatest number of possible grades of rhythm.

It will be appropriate to refer to the grades or steps of rhythm which we have found as *rhythm limens*. The number of limens is determined by the number of grades or steps between a perfect and disrupted rhythm. Apparently the rhythm limens are amenable to the Weber-Fechner psychophysical law, the mathematical constant being one third. For if we add one third of the value of a given limen to itself we obtain the value of the following limen. To this statement there are individual exceptions, and none of the empirical limens corresponds precisely to the theoretical; but the discrepancies are surprisingly small, as shown by the following figures. In these figures a minus sign indicates that the mathematical threshold is smaller, and the plus sign that it is larger than the empirically found limen. The discrepancies for rate 1.075 sec., continuous method, are, grade II., .0066 sec.−, for III., .0015+, for IV., .0178+, and for V., .0002 sec. +. For speed 0.57 sec. the corresponding deviations

are: .0001+, .0036−, .0051+, .0179+. The largest discrepancy is only .0179 sec. and the average of the eight only .0066. We find here, then, a striking extension of the psychophysical law to the field of rhythm and of esthetics.

When the results are averaged for the two speeds it is found that the theoretical values do not correspond very closely for grades IV. and V. Both are larger. This may indicate that the range to which the law applies in rhythm is quite limited, or otherwise that the subjects were overhasty in arriving at the determinations for the poorest rhythms.

The rhythm limens are not *sharp lines*, such as the thresholds of time differences. The moment the slightest difference in time is perceived we obtain a different limen. But the rhythm limens are extents, or time *spans*, having a certain breadth of time. Within these spans changes of time may be made without changing the grade or quality of the rhythm. The breadth of these rhythm thresholds varies considerably for different subjects, as may be seen by referring to the 'Range' columns in the tables. These refer to the intra-limen ranges, as distinguished from the inter-limen ranges, to which reference will be made in a moment. They have been calculated by subtracting the smallest from the largest single determination given by the subjects for any one grade of rhythm, so that, theoretically, they would represent 'neutral territory.' It is found that the average size of the rhythm *limen*, based upon the general average for the ranges, is, for the slower rate, .0499 sec., or 4.6 per cent. of the interval length; and for the faster tempo, .0222, or 4 per cent.; or 4.3 per cent. for both tempos (Table X.).

The ranges are fairly constant, the difference between the smallest and largest general averages being .0184 sec. for the slower tempo, and .0054 sec. for the faster, the M.V. for the two series being .0073 and .0017. They are less for the trochaic than the iambic pattern (in six out of ten cases), and for the discontinuous than the continuous method (except for grade III.).

The size of the intra-limen ranges may possibly also serve to indicate the degree of the subject's uncertainty. A very

large range would indicate that the subject is irregular, uncertain, or incapable of a fine appreciation of the differences between the various grades.

The rhythm limens do not merge directly into one another, but are separated by certain extents of time, which we may refer to as *inter-limen spans*. In the tables they are entered at the bottom as 'ranges between the grades.' They are obtained by subtracting the average of one grade from the average of the next higher. It is noteworthy that they are considerably smaller than the ranges within the grades. The average for the slower rate is .0336, or 3.1 per cent., and for the faster, .0151, or 2.7 per cent., which is 1.5 per cent. and 1.3 per cent. less, respectively, than for the corresponding ranges within the grades. This would seem to indicate that, while the limens are rather broad spans, they are quite sharply differentiated in consciousness. They do not vary much for the different grades (2.3, 3.1, 2.7 and 3.2 per cent.), but are relatively smaller for the faster tempo, as was the case with the previous ranges. There is no constant difference dependent upon pattern or method.

Are these inter-limen spans gaps, voids, in the rhythm curve? Probably not. They correspond to the imperceptible spaces between the successive sensation steps in the Weber-Fechner logarithmic curve of sensation, and thus parallel a well-known fact in this curve of sensation intensities.

A number of other minor results may be mentioned briefly. That there are considerable individual differences in the acuteness of discriminating grades of rhythm, can easily be seen by examining Tables I. to V. The acuteness is finer in the descending series, V, which approaches equality. In twenty out of twenty-five general averages for all grades these limens have a smaller value. This may be a practice effect, as the descending series always followed the ascending. Or the subjects may have been tardy in reaching a conclusion in both directions; this would tend to make the descending thresholds smaller.

Measured in terms of absolute time, the limens are all larger for the slower speed, but relatively to the length of

the measure (per cent. column) they are slightly smaller, except for grade V. In no case does the difference amount to more than 1.3 per cent., while the average difference for all grades in 0.6 per cent. That the limens should be smaller for the slower rate, in spite of the fact that all the subjects except one, as already remarked, preferred the faster tempo (because it heightened the feeling of rhythm, produced sharper sounds, etc.) may be due to the fact that the size of the minimal steps by which the tone was shifted was usually the same for the two tempos, namely, 1° at the beginning and 2° as the difference grew larger. Only occasionally were .5° steps used with the faster tempo. Vierordt found the keenness sensibly finer for the faster of two speeds. The difference for six series with the rate of 100 per minute was 23.5 per cent., as against 29.9 per cent. with the rate of 72 per minute.

The rhythm limens are smaller with the continuous than the discontinuous method, except for grade IV., the differences for the successive grades, based upon the per cents, being: 1.1 per cent., 0.9 per cent., 1.5 per cent., .1 per cent. (larger, for grade IV.), and .3 per cent., or an average of 1.2 per cent., for all grades. These results are based on the corresponding series, the trochaic pattern of the continuous and discontinuous methods. The experimental results correspond with the introspections. Seven subjects preferred the continuous method (only three the discontinuous), because it fixed the rhythm better (Wa.), or made it easier to apprehend (D., Wh., S.), or it held the attention better. The lenghening followed by a shortening produced a disturbance that was more easily apprehended than the disturbance caused by the lengthening alone (Wa.). One had to start the rhythm afresh every time with the discontinuous method. On the other hand, the discontinuous was considered better because there was not the same danger of losing the interval (F., W.), although one regarded the continuous better from the standpoint of the rhythm.

The limens are also somewhat smaller in the trochaic than the iambic pattern, the differences between the successive

grades being: .0142 sec., .0110, .0159, .0113 and .0050, or an average of .0115 sec. There is only one exception among the grades of the two tempos; viz., grade V., 0.57. Here again the introspections corroborate the experimental findings. All of the seven subjects who were questioned preferred the trochee, because the trochee rhythm was better or could be judged more easily (by five, F., D., McD., Wh., K.), or it was easier to attend to (F., Wh.), or it was preferred normally (F; but Wa. prefers iambic in poetry), or it was more soothing (D.); or the iambic was harsh and grating (K.), or it was tiring (F.), or unbalanced (S.) and unfamiliar (F.). The results point strongly to the conclusion that a trochee type of rhythm gives greater satisfaction than an iambic—a fact suggesting an important application in the field of musical and poetical esthetics.

II. The Difference Thresholds in the Perception of Time

As has already been said, the subjects were required to render both a time and rhythm judgment, particularly near the point where the intervals were about equal. They were requested to state after each liminal change at the beginning of the ascending and at the close of the descending series, whether or not any difference was noticed in the periodicity of the beats and in the quality of the rhythm. They also reported on the point at which the measures in the descending series appeared equal; *i. e.*, the point where the beats again came regularly. The results for the time judgments are given in Table XV.

For purposes of comparison the most reliable threshold value is probably the average of the first perceived difference (difference limen) of the ascending series, the last perceived difference of the descending series and the following equality judgment of the descending series. The constant errors incident to the particular direction of movement probably neutralize one another and thus disappear in the average for the three determinations. That an error of anticipation is present is indicated by the facts that four of the general

averages are larger and only one smaller for the ascending (∧) than the descending (∨) series. Accordingly the difference threshold was anticipated in both series. Naturally the equality threshold is smaller than either the ascending or descending thresholds, but there is one exception in the former series. It is noteworthy, however, that there is quite a number of individual exceptions: for some subjects the equality thresholds are sometimes larger than the difference thresholds, particularly in the ascending series. It is a matter of surprise that this should ever occur in the descending series, for here the point of equality necessarily followed the last difference limen. The explanation is that the subjects, on a few occasions, reversed their equality judgments and rendered a difference judgment without rendering a second equality judgment before the series of minimal changes was finished. This indicates a certain degree of subjective uncertainty or irregularity, which would probably have disappeared after practice, and which evidently would have occurred less frequently had the task of the subjects been restricted to rendering only a time judgment, instead of both a time and rhythm judgment. As the *time* problem was regarded as merely incidental to the *rhythm* problem, for purposes of comparison, check and control, no attempt was made to carry out a complete systematic determination of the temporal threshold. Besides their interest for comparative purposes, the results for time discrimination have an independent value under the conditions in force.

The following points summarize the more important results:

I. The difference thresholds for *time* are invariably *smaller* than the first rhythm limen (grade I.). The difference is not very large; in no case more than .0202 sec., or 1.9 per cent., and in four cases only .0145 sec., .0091 sec., .0072 sec. and .0018 sec. It thus appears that the time and rhythm thresholds are not identical; they do not precisely correspond. A change in time is apprehended before a change in the quality of the rhythm is felt. Moreover, there is this essential difference between the two thresholds: the time threshold is a

sharp edge or line, while the rhythm threshold is a broad range or extent, whose average duration, as already seen, amounts to 4.3 per cent. of the interval length (p. 112). That is to say, just as soon as the slightest change in the regularity or periodicity of the beats is apprehended, we have arrived at a temporal limen; as soon as another minimal difference is perceived we have a second time limen. In the case of rhythm, however, a certain appreciable increment must be added to the time limen before any effect on the rhythm is experienced. The perception of time and rhythm are, no doubt, closely related processes (Woodrow[1]), but they are not identical, even in the elemental perception of time differences. A 'temporal grouping' is obviously involved in rhythmical grouping, but the temporal grouping due to 'grouping by segregation' —*i. e.*, by increasing the temporal value of the preceding interval—is secondary. The primary and perhaps the only essential conscious datum in the experience of the rhythmizing subject is the fairly regular alternation of intensity differences (centroids[2]).

II. The difference threshold is relatively smaller for the longer interval. The size of the thresholds, based on the averages for the corresponding method (continuous), is .0323 sec. for the 0.57 sec. tempo, which is 5.6 per cent. of the length of the interval; and .0526 sec. for the 1.075 sec. tempo, which is 4.8 per cent. of the interval length. Relatively to the interval length, therefore, the limen for the longer interval is 0.8 per cent. smaller. In the rhythm series the corresponding difference was somewhat smaller, 0.6 per cent. This difference between the rates, as already explained (p. 114), is perhaps partly due to the fact that the minimal steps were in most cases absolutely the same for both tempos. This would tend to make the thresholds relatively larger for the faster speed. At the same time, since the subjective rhythm feeling was keener for the faster tempo and since this was the preferred tempo, we have here an additional indication that the time estimations and rhythm

[1] Woodrow, *op. cit.*, 50.
[2] Wallin, *op. cit.*, 9 ff.

appreciation are not identical and do not run precisely parallel.

III. If we compare the two methods, using the same pattern (— ⏑), it appears that the threshold for the continuous method is slightly smaller, the difference amounting to .0073 sec., or .7 per cent. The particular method is of less consequence in the estimation of time than in the differentiation of rhythm. For the rhythm judgments the limens were 1.2 per cent. smaller for the continuous than the discontinuous method (p. 114). Time differences can be appreciated relatively better than rhythm differences in short series. To secure the rhythmical swing requires a longer repetition of beats; and until a felt rhythmical swing is obtained the discrimination of grades of rhythm is not maximally effective. The continuous method, however, is superior in both the time and rhythm series.

IV. As to patterns, the limens are slightly smaller for the trochaic (— ⏑) than the iambic (⏑ —) type of measure. The difference is about the same in the two tempos, namely, .0046 sec., or .8 per cent. for 0.57 sec., and .0074 sec., or .77 per cent. for 1.075 sec. Here again the superiority of the trochee is seen to be more patent in the rhythm than the time series (p. 114).

It would be worth while to determine more fully whether the advantage of the continuous method and trochaic pattern arises primarily from the element of time or the element of rhythm. So far as our experimental results and introspections go, the rhythm is the determining factor.

V. As to the *size* of the time limens: the smallest, relatively to the interval length, is 4.5 per cent. (or .0489 sec. for speed 1.075 sec.); and the largest 6 per cent. (or .0346 sec. for speed 0.57 sec.). For the continuous method the thresholds are 5.67 per cent. for .057 sec. (or .0323 sec.), and 4.8 per cent. for the 1.075 sec. speed (or .0526 sec.). In three cases they are identical, 5.2 per cent. This is also the general average for all the tables, so that we may place the value of the differential limen in the estimation of time under similar conditions at 5.2 per cent. of the interval length. This deter-

mination falls within the limits of Vierodt's results. He found a difference limen of 2.5 per cent. with a tempo of 100 beats to the minute and 6.9 per cent. with a tempo of 42. These differences were needed to give 70 per cent. of correct answers.

Explanation of terms in the tables:

Grade I., II., III., etc. These represent five successive steps of increasing disintegration of the rhythmical unity. I. is relatively perfect rhythm; V., relatively disintegrated rhythm.

∧ *and* ∨. These signs indicate the directions in which the changes in the length of the variable interval are made by minimal steps. ∧ is the ascending or receding series of steps: it begins with equality, so that each step increases the difference. ∨, descending or approaching series; the return movement, toward equality; each step decreases the difference.

−1, −2 and −3, attached to the initials of the first subject in a different time series. The numerals indicate the time order of the various patterns for each subject. Thus for subject D., speed 1.075 sec., the experiments occurred in the following order: first sitting, continuous method, —⌣. Second sitting, discontinuous method, —⌣. Third sitting, continuous method, ⌣—. In all cases the experiments with the 1.075 sec. speed were finished before work was begun with the 0.57 sec. speed.

'Range' (within the grades). The 'range' is the distance between the smallest and largest determinations given by the subject for each grade, irrespective of the direction (∧ or ∨). It represents the difference between the shortest and longest individual determination, and thus indicates indirectly the degree of uncertainty of the subject's judgment. The averages of these ranges are based upon the number of counts, and not the number of subjects.

Range between the grades. These ranges correspond to the differences between the *averages* of the various grades. Thus the difference between the averages of I. and II. is .0428—.0318=.0110, or 2 per cent. of the length of the interval (0.57 sec.).

Continuous method. Here the disc revolved three times, thus permitting the subject to hear eighteen consecutive tones, nine loud and nine low, or six standard measures and three variables.

Discontinuous method. Here the tones were audible during each alternate revolution only.

Qualitative Limens of Rhythm

Table I

Duration of measures or intervals: 0. 57 sec. (time between the beginning of the tones).
Duration of tones: .095 sec. (duration of each degree of arc: .0095).
Pattern: 2-beat, —⌣. Continuous method.

Subject	Grade I			Grade II			Grade III			Grade IV			Grade V		
	∧	∨	Range	∧	∨	Range	∧	∨	Range	∧	∨	Range	∧	∨	Range
S.–2	.0285	.0332	.0475	.0736	.0570	.0475	.1045	.0855	.0285	.1187	.1045	.0190	.1425	.1235	.0190
C.	.0380	.0237	.0380	.0570	.0427	.0285	.0855	.0617	.0285	.1045	.0760	.0285	.1140	.0950	.0190
W.	.0190	.0285	.0285	.0332	.0475	.0285	.0475	.0665	.0190	.0712	.0760	.0285	.0950		
M.	.0665	.0570	.0665				.0950	.0855	.0285	.1235				.1140	
Wa.	.0190	.0285	.0285	.0285	.0380	.0095	.0380	.0475	.0095	.0475			.0570		
Ave.–2	.0342	.0342	.0418	.0481	.0463	.0285	.0617	.0693	.0228	.0926	.0855	.0253	.1021	.1108	.0190
D.–1	.0095	.000	.0095	.0190	.0142	.0095	.0380	.0380	.0380	.0475	.0617	.0380	.0760		
F.		.0190	.0190	.0095	.0285	.0190	.0285	.0380	.0190	.0475	.0475	.0000	.0570		
McD.	.0475	.0570	.0570	.0570	.0665	.0095	.0855	.0855	.0190		.1045		.1140		
Wh.	.0237	.0237	.0285	.0380	.0429	.0095	.0570	.0617	.0095	.0855	.0855	.0190	.1045		
K.	.0380	.0475	.0475	.0522	.0570	.0095	.0760	.0665	.0190	.1045	.0760	.0285	.1140	.0950	.0190
Ave.–1	.0297	.0294	.0323	.0351	.0418	.0114	.0570	.0579	.0209	.0712	.0750	.0214	.0931		
Ave.–1 and 2	.0319	.0318	.0370	.0416	.0440	.0199	.0593	.0636	.0218	.0819	.0802	.0233	.0976	.1029	.0190
Ave. ∧ and ∨ Range	.0318 = 5.5 per cent. Between grades I. and II., .0110, or 2 per cent.			.0428 = 7.5 per cent.			.0614 = 10.8 per cent. Between II. and III., .0186, or 3.1 per cent.			.0810 = 14.2 per cent. Between III. and IV., .0196, or 3.4 per cent.			.1002 = 17.6 per cent. Between IV. and V., .0192, or 3.4 per cent		

Table II

Duration of intervals and tones: same as in Table I.
Pattern: 2-beat, ⌣ —. Continuous method.

Subject	Grade I			Grade II			Grade III			Grade IV			Grade V		
	∧	∨	Range	∧	∨	Range	∧	∨	Range	∧	∨	Range	∧	∨	Range
F.–2	.0475	.0190	.0285	.0570	.0427	.0285	.0665						.0760	.0665	
McD.	.0570	.0760	.0190	.0855	.0427	.0380	.1235	.1045	.0190	.1424	.1140	.0285	.1615	.1330	.0285
Wh.	.0238	.0238	.0285	.0332	.0427	.0285	.0617	.0617	.0095	.0855	.0807	.0190	.1045		
K.	.0475	.0475	.0000	.0617	.0570	.0095	.0950	.0665	.0380		.0760		.1140	.0950	.0190
D.	.0285	.0475	.0190	.0427	.0617	.0385	.0617	.0807	.0285	.0855	.0950	.0190	.0950		.0190
Ave.–2	.0409	.0428	.0190	.0560	.0510	.0286	.0817	.0783	.0293	.1045	.0914	.0222	.1102	.0981	.0222
S.–1	.0380	.0380	.000	.0522	.0570	.0190	.0807	.0807	.0095	.1045	.0950	.0095	.1140		
C.	.0475	.0285	.0190	.0570	.0380	.0238	.0665	.0522	.0190	.0807	.0665	.0190	.0950	.0760	.0190
W.	.0570	.0475	.095	.0665	.0570	.0095	.0855	.0665	.0190	.0950	.0855	.0190	.1045		
M.	.0570	.0475	.095	.0665	.0617	.0095		.0760		.0974	.0950	.380			
Wa.	.0285	.0285	.0000	.0712	.0427	.0190					.0570		.0665		
Ave.–1	.0456	.0380	.0078	.0627	.0513	.0181	.0776	.0688	.0158	.0944	.0798	.0216	.0950	.0760	.0190
Ave.–1 and 2	.0432	.0404	.0134	.0593	.0511	.0233	.0796	.0735	.0225	.0994	.0856	.0219	.1026	.0870	.0206
Ave. ∧ and ∨ Range	.0418 = 7.3 per cent. Between I. and II., .0134 = 2.3 per cent.			.0552 = 9.6 per cent. Between II. and III., .0213 = 3.7 per cent.			.0765 = 13.4 per cent. Between III. and IV., .0160 = 2.8 per cent.			.0925 = 16.2 per cent. Between IV. and V., .0023 = 4 per cent.			.0948 = 16.6 per cent.		

For explanation of terms, see p. 119.

TABLE III

Duration of measures or intervals: 1.075 sec. (time between tones).
Duration of tones: .179 sec. (duration of each degree of arc: .0179).
Pattern: 2-beat, — ⌣. Continuous method.

Subject	Grade I			Grade II			Grade III			Grade IV			Grade V		
	∧	∨	Range	∧	∨	Range	∧	∨	Range	∧	∨	Range	∧	∨	Range
D.–1	.0716	.0537	.0179	.0984		.0179	.1343	.1029	.0716		.1790		.2148		
W.	.0716	.0179	.0716		.0447	.0537	.0895	.1074	.0358	.1074	.1611	.0537	.1790		
K.	.0179	.0358	.0179	.0537	.0626	.0358	.0895	.0985	.000	.1253	.1163	.0358	.1790		
Ave.–1	.0537	.0358	.0358	.0760	.0536	.0358	.1044	.0999	.0358	.1163	.1521	.0447	.1909		
S.–2	.1074	.1074	.0000	.1656	.1253	.0895		.1611		.2685	.2148	.0895		.2689	
McD.	.0626	.1074	.0537	.1134	.1879	.1253	.1969		.1253						
M.	.0537	.0358	.0358	.1253	.0656	.0895	.0895	.1074	.0358	.0895	.1600	.1074	.2506	.1611	.0895
Ave.–2	.0745	.0835	.0298	.1348	.1262	.1014	.1432	.1342	.0805	.1790	.1874	.0984	.2506	.2150	
F.–3	.0179	.0358	.0179	.0447	.0537	.0179	.0805	.0805	.0179	.1074			.1432	.1074	.0358
C.	.0537	.0179	.0358	.0805	.0447	.0537	.1074	.0716	.0358		.0895		.1253	.1074	.0179
Wh.	.0179	.0358	.0179	.0537	.0626	.0358	.0984	.0884	.0179	.1432	.1253	.0179	.1790	.1611	.0179
Wa.	.1432	.0805	.0716	.1790	.1163	.0716	.2327	.1611	.0895	.2864			.2685	.2148	.0716
Ave.–3	.0582	.0425	.0358	.0895	.0693	.0447	.1297	.1004	.0403	.1342	.1074	.0179	.1790	.1477	.0358
Ave.–1, 2, 3	.0621	.0539	.0338	.1001	.0830	.0606	.1258	.1115	.0522	.1432	.1490	.0537	.2068	.1813	
Ave. ∧ and ∨ Range	.0580 = 5.4 per cent. Between I. and II., .0335, or 3.1 per cent.			.0915 = 8.5 per cent. Between II. and III., .0271, or 2.5 per cent.			.1186 = 11 per cent. Between III. and IV., .0275, or 2.6 per cent.			.1461 = 13.6 per cent. Between IV. and V., .0479, or 4.4 per cent.			.1940 = 18 per cent.		

TABLE IV

Duration of intervals: 1.075 sec. (time between tones).
Duration of tones: .179 sec. (duration of each degree of arc: .0179).
Pattern: 2-beat, ⌣—. Continuous method.

Subject	Grade I			Grade II			Grade III			Grade IV			Grade V		
	∧	∨	Range	∧	∨	Range	∧	∨	Range	∧	∨	Range	∧	∨	Range
F.–1	.0716	.0179	.0537		.0537	.0358	.1253	.0716	.0895	.0895	.1253	.0358	.1790		
S.	.0895	.0984	.0179	.1253	.1522	.0716	.1790	.1969	.0358	.2237		.0358	.2685	.2327	.0358
Wh.	.0447	.0358	.0358	.0805	.0716	.0179	.1074	.1074	.0358	.1432	.1432	.0358	.1969		
Wa.	.0179	.0716	.0537	.0718	.1325	.1432	.1491	.2148	.1074	.2327		.0358	.2864		
Ave.–1	.0559	.0559	.0403	.0925	.1025	.0671	.1408	.1477	.0671	.1723	.1342	.0358	.2327	.2327	
C.–2	.1790	.1074	.0716		.1342	.0179				.2148	.1785	.0537	.2506		
K.	.0537	.0716	.0358	.0835	.1253	.0716	.1432	.1253	.0716	.1700		.0179	.2237	.1879	.0537
Ave.–2	.1163	.0895	.0537	.0835	.1297	.0447	.1432	.1253	.0716	.1924	.1785	.0557	.2371	.1879	.0537
D.–3	.0000	.0179	.0179	.0477	.0358	.0537	.0626	.0716	.0537	.1253	.0984	.0358	.1611		
McD.	.0776	.1253	.1074	.1208	.1611	.1432		.1969		.2506					
M.	.0895	.1253	.0358	.1313	.1611	.0537	.1969						.2506	.1969	.0716
W.	.0776	.0537	.0716	.0626	.0716	.0179		.1074		.1253			.1790	.1432	.0358
Ave.–3	.0612	.0805	.0582	.0906	.1074	.0671	.1297	.1253		.1671	.0984	0.358	.1969	.1700	.0537
Ave.–1, 2, 3	.0778	.0753	.0507	.0889	.1132	.0596	.1379	.1328	.0641	.1773	.1370	.0418	.2222	.1968	.0477
Ave. ∧ and ∨ Range	.0765 = 7.1 per cent. Between I. and II., .0245 = 2.2 per cent.			1.010 = 9.3 per cent. Between II. and III., .0343 = 2 per cent.			.1353 = 12.5 per cent. Between III. and IV., .0218 = 2 per cent.			.1571 = 14.6 per cent. Between IV. and V., .0524 = 4.8 per cent.			.2095 = 19.4 per cent.		

TABLE V

Duration of intervals and tones: 1.075 sec. (time between tones).
Duration of tones: .179 sec. (duration of each degree of arc: .0179).
Pattern: 2-beat, —⌣. Discontinuous method.

Subject	Grade I			Grade II			Grade III			Grade IV			Grade V		
	∧	∨	Range	∧	∨	Range	∧	∨	Range	∧	∨	Range	∧	∨	Range
C.–1	.0537	.0537	.0000	.0716	.0716	.0358	.1074	.1074	.0358	.1432			.1790	.1611	
McD.	.0626	.0179	.0537	.0984	.0806	.0537	.1432	.1163	.1790	.1790					
M.	.0895				.0716		.1790	.0268	.1611	.1074	.0984	.0179	.1432		
Ave.–1	.0686	.0358	.0268	.0850	.0746	.0447	.1432	.0835	.1253	.1432	.0984	.0179	.1611	.1611	.0179
F.–2	.0895	.0537	.0358	.1074	.0805	.0358	.1253	.1253	.0358				.1611		
D.	.0000	.0268	.0537	.0358	.0537	.0895	.0805	.1074	.0716		.1432		.1432		.0358
Wh.	.0358	.0537	.0179	.0626	.0805	.0358	.1074	.1074	.0179	.1432	.1253	.0179	.1790	.1611	.0179
Wa.	.1253	.1163	.0179	.1432	.1611	.0179		.1969	.0358	.2148	.2506	.0716	.2864	.2685	.0179
W.	.0358	.0358	.0000	.0537	.0805	.0537	.0805	.0805	.0179				.1611	.1253	.0358
Ave.–2	.0573	.0572	.0250	.0805	.0913	.0467	.0984	.1235	.0358	.1790	.1730	.0447	.1862	.1850	.0268
S.–3	.1790	.1074	.0716	.2327	.1432	.0895	.2864	.1969	.1253				.3043	.2506	.0537
K.	.0537	.0716	.0179	.0805	.0984	.0358	.1074	.1253	.0179	.1342	.1432	.0179	.1700		.0179
Ave.–3	.1163	.0895	.0447	.1566	.1208	.0626	.1969	.1611	.0716	.1342	.1432	.0179	.2371	.2506	.0358
Ave.–1, 2, 3	.0807	.0608	.0322	.1074	.0955	.0513	.1462	.1227	.0776	.1521	.1382	.0268	.1948	.1989	.0268
Ave. ∧ and ∨ Range	.0707 = 6.5 per cent. Between I. and II. .0307 = 2.8 per cent.			.1014 = 9.3 per cent. Between II. and III., .0330 = 3 per cent.			.1344 = 12.5 per cent. Between III. and IV., .0107 = 1 per cent.			.1451 = 13.5 per cent. Between IV. and V., .0517 = 4.8 per cent.			.1968 = 18.3 per cent.		

Tables of General Results

Table VI

Average Values for All Grades

Speed 1.075 *sec.*

	Grade I	Grade II	Grade III	Grade IV	Grade V
	Sec. %	Sec. %	Sec. %	Sec. %	Sec. %
Continuous method, — ⏑	.0580=5.4	.0915= 8.5	.1186=11.	.1461=13.6	.1940=18.
Continuous method, ⏑ —	.0765=7.1	.1010= 9.3	.1353=12.5	.1571=14.6	.2095=19.4
Ave.	.0672=6.2	.0962= 8.9	.1269=11.7	.1514=14.1	.2017=18.4
Discontinuous, — ⏑	.0707=6.5	.1014= 9.4	.1344=12.5	.1451=13.5	.1968=18.3
Ave.	.0684=6.33	.0979= 9.06	.1294=12.	.1497=13.9	.2001=18.5
Speed 0.57 *sec.*					
Continuous, — ⏑	.0318=5.5	.0428= 7.5	.0614=10.8	.0810=14.2	.1002=17.6
Continuous, ⏑ —	.0418=7.3	.0552= 9.6	.0765=13.4	.0925=16.2	.0948=16.6
Ave.	.0368=6.4	.0490= 8.01	.0689=12.1	.0867=15.2	.0975=17.1
General Ave. (both speeds)	.0526=6.36	.0734= 8.53	.0991=12.	.1182=14.5	.1488=17.8

Table VII

Comparison of Methods

Continuous$_A$

	Grade I	Grade II	Grade III	Grade IV	Grade V
	Sec. %	Sec. %	Sec. %	Sec. %	Sec. %
Speed 1.075, — ⏑	.0580=5.4	.0915= 8.5	.1186=11.	.1461=13.6	.1940=18.
Speed 1.075, ⏑ —	.0765=7.1	.1010= 9.3	.1353=12.5	.1571=14.6	.2095=19.4
Speed 0.57, — ⏑	.0318=5.5	.0428= 7.5	.0614=10.8	.0810=14.2	.1002=17.6
Speed 0.57, ⏑ —	.0418=7.3	.0552= 9.6	.0765=13.4	.0925=16.2	.0948=16.6
Ave.	.0520=6.3	.0726= 8.8	.0979=11.9	.1589=14.6	.1496=17.9
Discontinuous.					
Speed 1.075, — ⏑	.0707=6.5	.1014= 9.4	.1344=12.5	.1451=13.5	.1968=18.3

Table VIII

Comparison of Patterns

— ⌣

	Grade I		Grade II		Grade III		Grade IV		Grade V	
	Sec.	%	Sec.	%	Sec.	%	Sec.	%	Sec.	%
Continuous, 1.075	.0580	5.4	.0915	8.5	.1186	11.	.1461	13.6	19.40	18.
(Discontinuous, 1.075)[1]	(.0707	6.5)	(.1014	9.4)	(.1344	12.5)	(.1451	13.5)	(.1968	18.3)[1]
Continuous, 0.57	.0318	5.5	.0428	7.5	.0614	10.8	.0810	14.2	.1002	17.6
Ave.	.0449		.0671		.0900		.1135		.1471	
⌣ —										
Continuous, 1.075	.0765	7.1	.1010	9.3	.1353	12.5	.1571	14.6	.2095	19.4
Continuous, 0.57	.0418	7.3	.0552	9.6	.0765	13.4	.0925	16.2	.0948	16.6
Ave.	.0591		.0781		.1059		.1248		.1521	

[1] Not counted in average, owing to difference of method.

Table IX

Comparison of Ranges between the Grades

Speed 1.075 *sec.*

	Between Grades I. and II		Between II. and III		Between III. and IV		Between IV. and V	
	Sec.	%	Sec.	%	Sec.	%	Sec.	%
Continuous, — ⌣	.0335	3.1	.0271	2.5	.0275	2.6	.0479	4.4
Continuous, ⌣ —	.0245	2.2	.0343	3.2	.0218	2	.0524	4.8
(Discontinuous, ⌣ —)[1]	(.0307	2.8)	(.0330	3)	(.0107	1)	(.0517	4.8)[1]
Ave.	.0290	2.6	.0307	2.8	.0246	2.3	.0501	4.6
Speed 0.57 *sec.*								
Continuous, — ⌣	.0110	2	.0186	3.1	.0196	3.4	.0192	3.4
Continuous, ⌣ —	.0134	2.3	.0213	3.7	.0160	2.8	.0023	.4
Ave.	.0122	2.1	.0199	3.4	.0178	3.1	.0107	1.9
General Ave.		2.3		3.1		2.7		3.2

General averages for all grades of speed 1.075 sec.: .0336 sec., or 3.1 per cent of 1.075. M.V. = .0065.

General averages for all grades of speed 0.57 sec.: .0151 sec., or 2. 7 per cent. of 0.57. M.V. = .0037.

M. V. refers to the M. V. between the measurements for the different grades.

[1] Not counted in the average, owing to difference of method.

Table X

Comparison of Ranges within the Grades (Intra-limen Ranges)

(The ranges represent the difference between the longest and shortest times which occur in either of the series ∧ and ∨. The averaging is based on the number of counts in a column and not the number of subjects.)

Speed 1.075 *sec.*

	Grade I	Grade II	Grade III	Grade IV	Grade V
	Sec.	Sec.	Sec.	Sec.	Sec.
Continuous, —⏑	.0338	.0606	.0522	.0537	.0358
Continuous, ⏑—	.0507	.0596	.0641	.0418	.0477
(Discontinuous, —⏑)[1]	(.0322)	(.0513)	(.0776)	(.0268)	(.0268)[1]
Ave.	.0422	.0601	.0581	.0477	.0417
Speed 0.57 *sec.*					
Continuous, —⏑	.0370	.0199	.0218	.0233	.0190
Continuous, ⏑—	.0134	.0233	.0225	.0219	.0206
Ave.	.0252	.0216	.0221	.0226	.0198

[1] Not counted in the average.

Averages for all grades, speed 1.075 sec. = .0499, or 4.6 per cent. M.V. = .0073 sec.
Aves. for all grades, speed 0.57 sec = .0222, or 4 per cent.

Table XI

Attention Series

The determination of the rhythmical limens when attention is centered on the rhythmical impression.

Speed 1.075 *sec.*

	Grade I	Grade II	Grade III	Grade IV	Grade V
Continuous, —⏑	.0716[1]	.0887	.1248	.1288[2]	.1700
Continuous, ⏑—	.0716	.0939	.1342	.1387	.2028
Discontinuous, —⏑	.0805	.0822	.0885	.1454[2]	.1611[2]
Ave.	.0745L.	.0882G.	.1158G.	.1374G.	.1779G.
Speed 0.57 *sec.*					
Continuous, —⏑		.0506	.0570	.0760	.0918
Continuous, ⏑—		.0665	.1045	.0902	.0950
Ave.		.0585G.	.0807G.	.0831G.	.0934G.

[1] Individual count; not an average.

[2] Average for ∧ only. The above table gives only the general averages for the subjects from whom results were obtained. L. = loss of keenness. G. = gain.

Table XII

Comparison of losses and gains between the attention series, and the series in which no specific instruction was given to direct the attention to the ryhthm. G.= a gain in the fineness of rhythmical discrimination in the attention series; L=loss (based on general averages or the averages for ∧ and ∨ when no general average is available).

I	II	III	IV (0.57 sec. Continuous, —⌣)	V	Summary G.	Summary L.
	L.	G.	G. Continuous, ⌣—	G.	3	1
	L.[1]	L.[1]	G. 1.075 sec. Continuous, —⌣	G.	2	
L.[1]	G.	L.	G. Continuous, ⌣—	G.	3	1
G.	G.	G.	G. Discontinuous, —⌣	G.	5	
L.	G.	G.	L.	G.	3	2
					16	4

[1] Not based on averages

Table XIII

Table showing whether the lenghtening or shortening of intervals produced by displacing the tone was more noticeable.

Subj.	Speed 1.075 sec.						Speed 0.57 sec.					
	Continuous, —⌣			Continuous, ⌣—			Continuous, —⌣			Continuous, ⌣—		
	L.	S.	B.	L.	S.	B.	L.	S.	B.	L.	S.	B.
F.	Y	[1]				Y	Y	[2]		Y[3]		
S.	Y			Y	Y	Y[4]	Y[5]			Y		
D.	Y[6]			Y			Y	Y[7]		Y[3]		
C.	Y			Y[9]						Y[10]		
McD.		[11]		Y	[7]		Y[12]			Y	Y	
W.	Y[13]				Y[11]		Y[14]					Y
M.	Y			Y[15]					Y[16]	Y[17]		
Wh.	Y			Y			Y	Y				Y[19]
K.	Y			Y	Y[20]		Y[21]	Y			Y[22]	
Wa.	Y			Y[23]			Y[18]			Y[11]		

L., lengthening. S., shortening. B., both. Y. yes. (This indicates that the lengthening, or shortening of the intervals was noticed, or both, according to the column in which Y appears.

[1] Merely a suggestion of a shortened interval.

[2] Perceived it only when looking for it.

[3] Lengthening noticed, but cannot tell very well.

[4] Noticed only the longer at first, then both, then the shorter better, then both equally well, and then only the shorter.

[5] First only the longer, then both. Shorter at one time more distinct.

[6] First the longer, then both, then the shorter was clearer, then the longer.

[7] Noticed the shorter when attending to it.
[8] First the longer, then shorter also, then the longer only or better.
[9] First only the longer, then the shorter, then only the longer.
[10] First the longer, then both (longer clearer).
[11] Mostly.
[12] First the longer, then both, then the longer.
[13] First the longer, then both, then the longer usually.
[14] First the lengthening, then both, then only the lengthening.
[15] First the longer, then the shorter also; latter grew more distinct.
[16] Both, but shorter vague toward the close.
[17] First the longer noticed, then both (but the longer better), then the longer only.
[18] Noticed the lengthening first.
[19] Varied at first, then noticed the longer, and then both.
[20] Noticed the shorter when suggested, then more easily.
[21] Noticed the longer, then the shorter (but not so well).
[22] Shorter first, then the longer also, but the shorter was usually clearer.
[23] Noticed the lengthening first, then both, then the shortening more clearly, then both equally well then the shortening less, then only the lengthening.

TABLE XIV

Introspections on the rhythmical impressions produced by the loud and soft resonator tones.

Subj.	Preliminary introspections (speed 1.075 sec.).			Preliminary and later introspections.		Later reports on what instrument or experience gives the strongest rhythmical feeling.[1]	Did you have any method of keeping or aiding the rhythmical impression?
	Were weak and strong beats felt?	Any rhythmical feeling?	Were the beats regular?	What instrument gives a similar rhythm?	What a stronger rhythm?		
F.	Yes.	None: too slow (later very good).	Yes.	Organ.	Drum taps.	Dancing.	No.
S.	?	A suggestion. Rather slow.	Yes.	Bell.	Drum taps, much better.	Two-step, or puffing of engine.	No. Beat time only in attention.
D.	Yes.	Yes.	Yes.	Noise of railway train or dentist's saw.	Drum taps.	Dancing, good waltz.	No.
C.	Yes.	Yes (later very good)	Yes.	Bell or clock.		Dancing. Music.	None for 0.57 sec. Beat with feet and hands once for 1.075.
McD.	Yes.	Yes (?)[2]	Yes.		Dactylic hexameter.		None noticed.
W.	Yes.	Yes.	Yes.	Metronome.	Drum taps.	Music. Quickstep. Band.	Tapped with finger sometimes.
M.	Yes.	Yes, when suggested.	Yes.	Tolling of large bell.	Drum taps.	Dancing.	Moved hand sidewise.
Wh.		Yes, second time (later very good).	Yes	Bell.	Drum taps, much superior.	Drum or trombone in band. Rapid quickstep.	
K.	Yes.	Yes.	Yes, slowing toward close.			Band music. Seeing dancers and beating time with the music.	Beat time with hand and foot.
Wa.	Yes.	Yes.	Yes.	Loud clock.		From operetta with orchestra, and seeing players sway.	Did not notice. Needed no aid with 0.57.

[1] In a later experiment on preferred tempos with the metronome, the same question was asked six other subjects, with the following results: (1) Orchestra, good martial music or bugle call. (2) Band music (good rhythm from poetry also). (3) Drum taps. (4) Metronome.

[2] Apparently has a poor sense of rhythm.

Table XV

Time Judgments.

The difference thresholds in the perception of time. The results are all for grade I., and represent the general averages for all the subjects from whom data were obtained.

Speed 0.57 sec., continuous, — ⌣.

∧	∨	≙	Ave.	
.0289	.0327	.0285	.0300, or 5.2 per cent.	(Results for the Rhythm Threshold.)
			Same, ⌣ —.	
.0356	.0399	.0285	.0346, or 6.0 per cent	(.0418, or 7.3 per cent.)
			Speed: 1.075 sec. Continuous, — ⌣.	
.0514	.0502	.0487	.0501, or 4.6 per cent.	(.0580, or 4.6 per cent.)
			⌣ —.	
.0418	.0714	.0526	.0553, or 5.1 per cent.	(.0765, or 7.1 per cent.)
			Discontinuous, — ⌣.	
.0544	.0642	.0500	.0526, or 5.2 per cent.	(.0707, or 6.5 per cent.)

∧ ascending series. ∨, descending series. ≥, equality judgments in descending series.

EXPERIMENTAL STUDIES IN JUDGMENT

Judgments of the Comic

BY H. L. HOLLINGWORTH[1]

Columbia University

Reactions to comic situations are notoriously dependent on individual temperament, mood and circumstance. So much is this true that few attempts to control these variable factors experimentally or to measure in any way the subjective element in the response to the comic have been reported. One of the most striking things about such reactions is the important rôle played by adaptation to the stimulus. There is a decided change in the reaction whenever the element of novelty, surprise or shock is eliminated, or whenever the climax is in any way anticipated or remembered. But the mere fact that these factors are subjective or highly variable is no reason why they should not submit to measurement by appropriate methods. Nothing would seem to be more subjective and variable than judgments of scientific eminence, of the relative beauty of picture postals, the legibility of handwriting, family resemblance in penmanship or the persuasiveness of advertisements. Yet all these have yielded to methods of measurement and the coefficients derived have been sometimes objectively verified. Thus the writer has found that subjective judgments of the relative persuasiveness of advertisements actually check up with the keyed results of those advertisements when they appeared.

The variable elements in judgments of the comic fall into two chief groups:

1. *Variabilities in the observer.*—No two individuals are supposed to react in quite the same way to the same comic motive. Associations leading into remote quarters of personal experience, recent happenings and present apperceptive

[1]The writer was assisted in the conduct of the experiment and in the computation of the 'waxing' and 'waning' results reported in section II. by Miss E. V. Dunnett, A. B. Barnard, '10.

attitudes most diverse in kind, emotional complexes varying widely in color and complexity, seem nowhere else to be so potent as here. One individual never wearies of a favorite jest which for others soon loses its flavor. Another is perennially amused by puns which bore his friends; some of us always 'see the funny side' while others never seem able to 'catch the point.' Even the fact of racial differences, so difficult to detect in most traits, is apparent here.

2. *Variabilities in the comic situation.*—Every-day observation shows that some jokes lose their relish immediately after the first hearing while others will endure a considerable number of repetitions before adaptation takes place or even will improve with increased familiarity. There are differences in speed, in the number of repetitions required, as well as in the final degree of adaptation, which may be easily measured, analyzed and balanced against each other. The semi-metaphysical classics have had much to say concerning the process of adaptation, but these statements have been usually spun out of exceeding fine yarn. The only strictly experimental ventures into the field of the comic, to the writer's knowledge, are the questionnaire of Hall and Allin[1] and the tests of Martin[2] by the various psychophysical methods. Neither of these papers throws any clear light on the questions proposed in the present inquiry. The purpose of this study is to seek answers to some or all of the following questions concerning the two groups of variabilities outlined above.

I. Judgments of the comic:

1. What differences are found among a group of observers, in the apperception, appreciation and evaluation of a given series of comic situations?

2. What variability does the same observer show from time to time in his reactions to the same motive?

3. How does the average variability of the same observer compare with that of a group of observers?

4. Is the consistency of an individual in any way corre-

[1] Hall and Allin. On Tickling and Laughing. *Amer. Jour. of Psychol.*, **9**, Oct., 1897, pp. 1–41.

[2] Martin, L. J. Psychology of Esthetics. *Amer. Jour. of Psychol.*, **16**, 1905 pp. 35–117.

lated with his own agreement with the group average? Is his consistency in any one task characteristic of his performance in a different task?

5. What relation does the character or quality of the motive bear to the degree of variability in these cases?

6. Does the judgment of the comic display any peculiar characteristics? What contribution to the general theory of judgment can such an investigation make?

II. The psychology of adaptation to the comic:

1. Do all jokes decrease in funniness with repetition?

2. If so, do they all grow stale together or do some become chestnuts more quickly than others?

3. Or are there perhaps (*a*) waxing jokes, in which each repetition provokes an even greater appreciative reaction, (*b*) waning jokes which decline steadily in affective value, and (*c*) static ones, which undergo no transformation in merit. What are the chief 'categories' of the comic, and what is the behavior of each type in the chestnut process?

III. Can such experimental investigation contribute in any way toward the 'theory of the comic' which hitherto for the most part has been treated only in an analytical and introspective way?

Method and Procedure

The comic situations were in this experiment afforded by a series of thirty-nine jokes, ranging rather widely in character and merit, culled from a collection to which several individuals contributed their choicest specimens. The attempt was made to secure, as far as possible, 'uncirculated' jokes, so that each observer would begin at an approximately identical point of adaptation. It need hardly be remarked that this condition was not rigidly fulfilled. Tradition asserts that there are only five distinct jokes in the world, but for our purpose 10 chief categories were chosen as comprising the principal types disclosed by an analysis of the whole collection and several representatives were selected for each category. The classification seems to cover most of the distinct types pointed out in the analytical treatises. In order to classify the jokes fairly, ten disinterested observers independently

sorted the group of 39 under the 10 headings, and in the final interpretation of the results with respect to the adaptation process the majority of these 10 votes decided the category under which the joke was thrown (see Table IX.). The jokes were type-written on cards of the same size, and in order to leave the interpretation of the situation entirely to the reader, the titles, often suggestive of one interpretation rather than another, were omitted.

The plan adopted for securing the judgments was a modification of the order of merit method so successfully employed by Cattell,[1] Sumner,[2] Norsworthy,[3] Wells,[4] Downey[5] and Thorndike.[6] The present modification, which may be designated the *group method* as contrasted with the *strict order method* possesses several advantages which justify its further development. Instead of arranging the material in strict order of merit the observer placed them in 10 piles, according to their 'degree of funniness.' In the first pile were placed the superior jokes, in the tenth the poorest ones, while the intermediate piles represented gradations of merit from best to poorest. No instructions were given as to the amount of difference represented by these successive piles, nor as to the number of cards to be placed in each.[7]

Ten observers took part in the experiment, all of whom were women, students in the Barnard laboratory, with one and a half year's work in psychology. When the average position of each card for the ten observers was calculated, the 39 jokes

[1] Cattell, J. McKeen. A Statistical Study of American Men of Science. *Science*, 24, Nov. and Dec., 1906. See also his Statistics of American Psychologists. *Amer. Jour. of Psychol.*, 14, 1903, pp. 310–328.

[2] Sumner, F. B. A Statistical Study of Belief. PSYCHOL. REV., 5, 1898, pp. 616–631.

[3] Norsworthy, N. The Validity of Judgments of Character. Essays Philosophical and Psychological in Honor of William James, 1908, p. 551.

[4] Wells, F. L. On the Variability of Individual Judgment. Essays Philosophical and Psychological in Honor of William James, 1908, p. 511.

[5] Downey, J. E. A Preliminary Study of Family Resemblance in Handwriting, Psychol. Bulletins, Univ. of Wyoming, No. 1, 1910.

[6] Thorndike, E. L. Handwriting. *Teachers College Record*, 11, No. 2, 1910, p. 57.

[7] Just what differences in degree this classification would show, and whether or not a Weber's law principle is present in the discrimination of these affective stimuli, would be an interesting topic for investigation.

could be arranged in a strict order of merit according to their respective averages. The advantages of this group method are several.

It is much quicker than the strict method, less fatiguing and monotonous to the observer, yet correlates closely with results from the same observers by the strict order method. Further, the method gives opportunity to observe any changes in value of the group as a whole. Thus by multiplying the number of cards in a given group (say 7) by the position of that group (say number 9) and adding these products for all ten groups a figure is obtained which gives some measure of the total value of the series for a given individual or group. Now if the cards are arranged a second, third, fourth, etc., time by the same observers, these sums will indicate the change in total value of the series during the successive trials (see Plate III.). This figure is of course not in any sense an absolute measure. It is conditioned by shifts in the individual's standard of value, by his personal variability of judgment, by the variation in standard from individual to individual, and by the fact that no card can be thrown higher than the first nor lower than the last pile. Nevertheless it affords an interesting and suggestive index of the total series behavior which the strict order method cannot yield. It will be shown later that the M.V. (mean variation) in such experiments always bears a constant ratio to the number of places into which the objects are to be sorted, so that the relative variability is the same here as in the strict method.

There may be, in the *group method*, a certain tendency to arrange stimuli according to qualitative or type resemblance, which might to a degree disturb the judgment of merit,—a tendency, that is, to put all puns in the same pile, etc. But there is no evidence in the results that such an inclination has in any way operated. Moreover the tendency is just as strong, in the *strict order method*, to put qualitatively similar stimuli in the same region of the scale. Thus Wells found that in arrangements of picture postals according to preference there was a tendency to place near each other cards bearing similar scenes, color schemes, etc. It is conceivable

that even in arranging individuals with respect to scientific eminence, contiguity in space or similarity of field or method may operate as a more or less significant associative factor in determining relative position. But since these factors also help determine the individual's actual judgment of merit, they need not be supposed to warp that judgment in any undesirable way.

In the present experiment each of the 10 observers arranged the cards five successive times, the trials being a week apart. This plan thus gave data for investigating the variability of the group, of the individual, of the total value of the series and of the behavior of each card under the influence of repetition. Both Wells and Downey have shown that a week is ample time for the elimination of any great disturbance through the memory factor in the successive trials.

Results

Results will be treated under three headings: First, with respect to judgment; second, with respect to the 'chestnut' process; third, with respect to the theory of the comic.

Table I. shows the average value and the M.V. of each card (numbered in a random order) for the 10 observers in the five successive trials, and indicates also the final position of each joke in each trial. Adding each column of average positions gives for each trial the total value for the series and averaging the M.V. column gives the average M.V. of the 10 observers for each of the five trials. Thus the total value of the series amounts to 214.1 points in the first trial, and drops in each following trial, through 216.5, 230.7, 235.0, to 239.9 in the fifth. That is to say, the jokes as a whole wane in value, assuming that the average standard of the group remained approximately constant throughout. The shift is not so great, however, as we might expect. Since the observer was instructed to have always ten piles, the jokes could not all begin in pile 1. There would be at least one in each of the other nine piles, making the highest possible number of points (with 30 cards in pile 1 and one card in each pile from 2 to 10) 84. Nor could the cards all fall to the bottom pile, since there

must needs be one remaining in each of the other nine, so that the lowest possible limit would be 240. Thus the maximum possible fall would be from 84 to 340 points, or a loss of 256 points. The actual drop is only from 214 to 240, or 26 points, approximately one tenth of the possible fall. This slight change is probably due to the fact that the cards originally in pile 10 could never be thrown lower. Yet those in the piles above, while becoming weaker in their laughter-provoking quality, preserved their superiority to the pile below and could not be assimilated with them. This means then that the general standard of the individual would fall from trial to trial, the lowest group (10) representing each time an absolutely lower value. This drop would extend itself all the way up the scale, so that the jokes which remain on a level throughout the experiment really drop, those rising do not rise so much as they seem to, while those dropping fall more rapidly, with respect to absolute value, than they appear to. The total waning coefficient does not then measure the absolute change in value of the group, but it is nevertheless a significant figure in comparison of group with group or individual with individual.

The average M.V.'s for the successive trials show no change in magnitude, remaining always between 2.1 and 2.3 places, or .22 the total number of positions. The significance of this ratio will be referred to later.

No.	1st Trial			2d Trial			3d Trial			4th Trial			5th Trial		
	Av.	M.V.	Pos.	Av.	M.V.	Pos.	Av.	M.V.	Pos.	Av.	M.V.	Pos.	Av.	M.V.	Pos.
1	6.4	2.60	30	6.8	2.44	31	7.3	2.24	34	7.4	1.60	34	7.6	1.96	32
2	5.2	2.24	14	6.6	2.12	28	6.1	2.10	21	5.2	2.34	12	6.8	2.04	23
3	5.5	3.10	18	4.9	2.32	13	5.4	2.70	13	4.9	2.21	8	5.9	2.52	16
4	5.6	3.12	19	7.0	2.20	35	7.6	1.68	36	7.0	2.40	31	7.1	1.80	28
5	5.4	2.20	16	5.0	1.80	14	6.7	1.76	27	5.5	2.30	13	5.9	1.71	15
6	5.9	1.90	22	3.6	3.04	2	4.3	2.56	6	3.8	1.66	4	4.5	2.40	5
7	7.0	2.00	37	7.7	1.82	38	7.3	2.10	33	8.6	1.96	39	8.2	1.80	39
8	3.8	2.20	4	4.0	2.30	5	3.9	2.10	4	5.8	1.84	16	4.9	2.68	10
9	3.2	1.40	2	5.2	2.44	15	5.8	2.10	17	6.4	3.64	22	6.5	2.20	20
10	6.4	1.52	28	6.1	1.70	24	5.6	2.80	16	6.1	2.85	19	6.2	2.06	18
11	6.9	1.98	35	7.3	1.70	37	6.9	1.43	30	7.5	1.90	35	8.1	1.50	37
12	4.3	2.08	9	4.2	2.08	7	5.1	2.32	10	5.2	2.08	10	4.0	1.80	3
13	6.6	2.08	32	6.3	1.76	25	6.9	1.80	31	6.8	2.62	28	7.7	1.63	33
14	6.3	1.84	27	5.8	2.08	21	5.8	2.56	19	6.1	2.72	20	7.2	2.40	30
15	6.4	2.20	21	5.7	2.70	19	6.2	1.40	22	6.8	1.80	27	6.9	1.92	25
16	6.2	2.60	25	6.6	2.76	30	6.2	2.96	23	5.7	3.26	15	6.5	3.10	21
17	7.2	2.36	38	7.7	1.88	39	8.3	1.78	39	8.3	2.50	38	8.2	1.56	38
18	5.2	2.44	15	6.3	2.30	26.5	6.8	2.26	29	7.3	2.30	33	7.2	2.52	31
19	6.8	2.08	34	6.8	2.12	34	6.8	2.04	28	6.9	2.10	29	6.5	2.00	19
20	4.2	1.88	7	4.3	1.56	8	5.5	2.20	15	5.8	2.14	17	4.8	1.84	7
21	4.0	1.80	5	4.6	2.40	11	4.8	1.60	9	4.3	1.62	5	4.2	1.84	4
22	4.8	2.50	11	6.0	1.40	23	4.6	1.52	8	5.6	1.80	14	5.2	1.96	11
23	2.2	1.48	1	2.0	1.20	1	1.9	0.90	1	2.6	1.66	1	2.2	1.12	1
24	6.1	2.34	24	6.8	3.04	33	7.1	3.18	32	6.5	3.10	25	7.0	2.80	26.5
25	5.0	2.40	12	5.8	2.74	22	6.4	2.20	24	6.5	2.20	23	6.8	2.63	24
26	5.5	2.60	7	4.4	1.88	10	5.4	2.60	12	2.9	2.13	2	4.8	2.00	8
27	3.7	2.84	3	3.9	3.16	4	3.8	2.98	2	3.7	2.44	3	3.3	2.22	2
28	5.7	3.30	20	5.7	2.76	29	5.8	2.74	18	6.7	2.80	26	6.6	2.92	22
29	4.8	2.40	10	5.2	3.40	17	5.3	2.90	11	4.6	2.40	7	5.2	3.44	12
30	4.1	2.52	6	4.1	1.90	6	3.9	1.72	3	5.2	2.24	11	4.8	2.20	9
31	6.5	3.20	31	6.6	2.40	29	6.5	2.10	25	7.7	1.38	37	7.8	1.84	35
32	5.0	3.20	13	4.7	2.18	12	4.5	2.70	7	6.4	2.92	21	5.5	3.00	13
33	6.9	2.94	36	6.6	2.60	32	7.5	2.20	35	7.0	2.80	32	7.0	2.80	26.5
34	7.2	2.72	39	7.0	2.60	36	7.9	3.17	37	6.9	2.80	30	7.8	2.38	36
35	5.8	2.40	21	5.4	2.48	18	5.9	2.50	20	6.5	2.40	24	5.9	2.72	17
36	4.2	2.04	8	3.7	1.56	3	4.2	1.86	5	4.5	2.10	6	4.6	1.49	6
37	6.7	2.50	33	6.3	2.30	26.5	8.0	1.80	38	7.5	2.40	36	7.7	2.17	34
38	6.0	1.80	23	5.3	1.76	16	5.5	2.20	14	4.9	2.83	9	5.7	1.98	14
39	6.2	2.64	26	4.4	1.68	9	6.6	2.02	26	5.9	2.50	18	7.1	2.46	29
	214.1 Sum	2.3 Average.		216.5 Sum.	2.2 Average.		230.9 Sum.	2.2 Average.		235.0 Sum.	2.3 Average.		239.9 Sum.	2.1 Average.	

Variability within the Group

Likes and Dislikes.—If the cards be arranged in a final order of merit for each trial and the M.V.'s of the best cards compared with those of the poorest, that is, if the M.V.'s of the top and bottom of the series be compared, the members of the group are found to agree more closely at the top than at the bottom. Table II. gives the M.V. for the first and last ten places in each of the five trials. Inspection shows two facts. First, that the M.V. for the top groups, taken either by 5's or 10's, is less than for the lower. Thus the average M.V. for places 1–10 is 2.03 compared with 2.22 for places 30–39. The M.V. of places 1–5 is 1.97 compared with 2.09 for places 34–39.

Table II

Av. M. V.'s, 10 Observers, 5 Trials

Pos.	Trial 1	Trial 2	Trial 3	Trial 4	Trial 5
1	1.48	1.20	0.90	1.66	1.12
2	1.40	3.04	2.98	2.12	2.22
3	2.84	1.56	1.72	2.44	1.80
4	2.20	3.06	2.10	1.66	1.84
5	1.80	2.32	1.86	1.62	2.40
6	2.52	2.40	2.56	2.10	1.49
7	1.88	2.08	2.70	2.40	1.84
8	2.04	1.56	1.52	2.21	2.00
9	2.08	1.68	1.60	2.83	2.20
10	2.40	1.88	2.32	2.08	2.68
—					
—					
—					
—					
30	2.60	2.76	1.43	2.80	2.40
31	3.20	2.12	1.80	2.40	2.52
32	2.08	3.04	3.18	2.80	1.96
33	2.50	2.44	2.10	2.30	1.63
34	2.08	2.12	2.24	1.60	2.17
35	1.98	2.40	2.20	1.90	1.84
36	2.94	2.20	1.68	2.40	2.38
37	2.00	1.70	3.16	1.38	1.50
38	2.36	1.88	1.80	2.50	1.56
39	2.72	1.82	1.78	1.96	1.80

Second, this difference becomes smaller with each repetition, the differences between the M.V.'s of 1–5 and 34–39 being successively .46, .23, .21, .13, .05, and between the M.V.'s of 1–10 and 30–39, being .39, .24, .17, .10, .01. Generalizing we may say that in the beginning individuals agree more

closely on the good than on the poor, but that with successive repetitions this difference disappears (see Table III.).

TABLE III

AVERAGES FROM TABLE II

	1	2	3	4	5	Average
Av. 1–5.....	1.94	2.23	1.91	1.90	1.87	1.97
Av. 34–39.....	2.40	2.00	2.12	2.03	1.82	2.09
Difference.....	+ .46	− .23	+ .21	+ .13	.05	
Av. 1–10.....	2.06	2.08	1.97	2.11	1.96	2.03
Av. 30–39.....	2.45	2.32	2.14	2.21	1.97	2.22
Difference.....	+ .39	+ .24	+ .17	+ .10	+ .01	

This first relation seems to be a usual one in judgments of this subjective character,—of preference, beauty, persuasiveness, etc. Thus in Wells' study of picture postals, although the author does not call attention to the fact, the figures yield the following result. For places 1–5 and 45–50, the M.V.'s are much alike, being respectively 8.7 and 8.5. For places 1–10 the M.V. is 8.5 while for 40–50 it is 10.2. For 1–15 it is 9.5 as against 10.3 for places 35–50, etc. In a yet unpublished study by the writer, of the judgments of twenty men with respect to the persuasiveness of 50 advertisements the same relation holds, the average M.V. for places 1–10 being 9.8 as against 10.8 for places 40–50.

This seems to be a not uninteresting point in the psychology of judgment, the significance of which is not clear. Wells finds that for repeated trials by the same individual the reverse situation holds, the same individual agreeing closer at the bottom of the scale than at the top, and the suggestion is made that this may mean that we are more certain of our dislikes than of our preferences. Giving the present relation a somewhat analogous interpretation, it may mean that although a single individual may be more certain of his antipathies, a group of individuals will resemble each other more in their preferences than in their aversions.

Or the relation may mean simply that we attend to things possessing positive quality. That here where the expression of the judgment is in terms of preference we attend

more strongly to the end in which our preferences really lie. But that this is not true for all individuals will be later pointed out. Dearborn[1] finds judgments of unlikeness easier to make than judgments of similarity, and Downey finds some evidence for the same relation, although the average of her results confirms the statement of Wells. But the judgment of preference is qualitatively different from the judgment of resemblance, the one being based on feeling-tone, the other on more restricted perceptual factors.

Another possible interpretation of the data is that the differences between the superior cards, at the top of the scale, are greater than those of the mediocre at the bottom. This was clearly shown by Cattell to be the case in judgments of scientific achievement. Thus "The figures show that the average differences[2] between the chemists who are in the first tenth are about eight times as great as between the chemists toward the middle of the list and about twelve times as great as between the chemists toward the bottom of the list" But there are at least three reasons for believing that there is considerable change in attitude when the same observer turns from arranging men according to merit to arranging simple stimuli according to affective tone. The difference lies in the fact that part way down the scale, in the latter case, the expression of judgment changes from terms of decreasing preference into terms of increasing positive dislike, whereas probably few scientists who would get into a total group would be rated as positively bad, the judgment being expressed rather in terms of more or less merit. Arrangements of scientific merit resemble the scale of sensation intensities, varying always in terms of degree, while arrangements of preference resemble the gradation of feelings from the positive pole through a region of indifference to a decided negative pole.

In the second place the suggestion that the smaller variability in the upper ranges depends on objective differences in

[1] Dearborn, G. V. N. Notes on the Discernment of Likeness and Unlikeness. *Jour. of Phil., Psychol., and Scientific Methods*, 7, 1910.

[2] Measured inversely by the size of the probable errors and directly by the difference in grade.

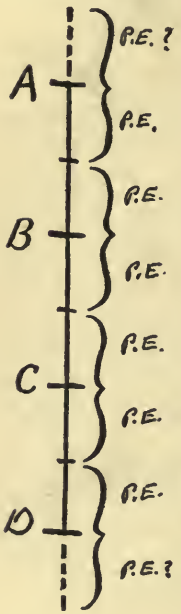
the stimuli is contradicted by the fact that in the successive arrangements by the same individual four of the ten observers were more consistent in the lower range than in the upper, and this would hardly be expected if the differences between the cards in this lower range were actually smaller than in the upper. Furthermore if something like Weber's law holds for judgments of affective tone as well as for sensation intensity, differences in the upper range would have to be greater in order to yield equal variability, and considerably greater if the variability is still smaller. The whole question of this closer group agreement in the upper ranges seems to merit further investigation and especially, the tendency of the differences to become uniformly smaller in successive trials.

Another striking relation brought out by the comparison of various order of merit arrangements of stimuli on the basis of such affective factors as preference, beauty, persuasiveness, funniness, etc., is the constancy of the ratio of the average M.V. for the series as whole to the number of possible positions in the range. If by M.V. we designate this average variability and by P the total number of positions in the scale then M.V./P is, with various kinds of material, with different groups of observers and with a widely ranging value for P, usually .20, and with high reliability. The following table exhibits this relation in such material as the writer has at hand.

TABLE IV

Material	Trait	Observer	P	M.V.	M.V./P
1. 4 advertisements	Persuasiveness	10 men	4	.8	.200
2. 5 advertisements	Persuasiveness	10 men	5	.98	.196
3. 39 jokes	Funniness	10 women	10	2.2	.220
4. 10 advertisements (av. of 4 sets)	Persuasiveness	10 women	10	2.3	.230
5. 10 advertisements (av. of 3 sets)	Persuasiveness	20 mixed	10	2.5	.250
6. 20 advertisements (av. of 2 sets)	Persuasiveness	50 mixed	20	4.3	.215
7. 20 photographs	Various traits	10 women	20	3.6	.180
8. 39 jokes	Funniness.	10 women	39	8.03	.205
9. 50 appeals	Strength.	20 women	50	10.5	.201
10. 50 picture postals (Wells)	Beauty.	10 mixed	50	10.7	.201

That is to say, the M.V. is always about one fifth of the total number of possible places, or the P.E. (probable error) assuming a normal distribution, about .168 or about one sixth of the range. The evidence seems to the writer too strong to permit of explanation in terms of mere coincidence. Of course if the material had been the same throughout, the only variable being the number of places into which it was sorted, this is just what we might expect, for the relative P.E. would remain constant, the absolute P.E. depending on the fineness of the grades of distinction. But we have here eight distinct sets of material, judged in termsof a considerable range of traits, by widely differing groups of observers, both as to sex, training, interest and number. The only constant factor is that the judgment is always based on the affective reaction to the stimulus. And we find that in every case the probable error is approximately one sixth of the range. (It would probably be slightly larger if it were not for the fact that the end error tends to reduce the variability of the extreme upper and lower positions.) Assuming that the M.V.'s were equal in all parts of the range (and they do not vary greatly), and allowing a P.E. in both directions from both the upper and lower extremes, the total range would then be divided into four sections, each separated from its neighbor by the respective P.E.'s, somewhat as follows. This would mean that, so far as the average judgment of the group of observers is concerned, there are only four distinct grades of difference or merit in the material, only four shades of distinction on which the group would, in the long run, agree, these grades corresponding to the sections lying about *A*, *B*, *C* and *D* as central tendencies.



This situation is curiously analogous to that disclosed in judgments of the same observer, where practice shows that about four distinctions of certainty, clearness, etc., are all

that can be comfortably and accurately made. The same thing that holds for the variability of the individual holds for the variability of the group. And the fact that the law holds for such different kinds of material and traits argues an interesting resemblance between the judgments involved in such affective discriminations.

The size of this ratio M.V./*P* would become smaller as the material came to be selected so as to disclose more pronounced or more objectively measurable differences. Thus in judgments of resemblance of penmanship which are supposedly more directly perceptual and objectively verifiable in kind, Downey finds M.V.'s which, if arranged as below, according to the range of possible positions, would yield an M.V./*P* value of about .163, or a probable error of about .130, meaning that while there are only about four clearly marked grades of beauty, funniness, persuasiveness, etc., there are about five clearly marked degrees of resemblance.

TABLE V

VARIABILITY OF JUDGMENTS OF SIMILARITY (DOWNEY)

P	M.V.	M.V./*P*
20	3.31	.165
34	5.33	.157
37	6.22	.168
	Average M.V./*P* = .163	

This ratio M.V./*P* can then be used as a reliable index of the objective character of judgments and with greater accuracy than the crude M.V. employed by Wells. Using this ratio the objectivity of his three classes of judgments would be,—preference .201, colors .086, and weights .141, showing that the judgments of weight order were more subjective than those of color order, thus reversing the order assigned by Wells.

INDIVIDUAL VARIABILITY

Turning now from the variability of the group to the successive judgments of the individual, several questions suggest themselves;

1. Does the individual, as the group, judge the upper range positions with greater regularity than the lower?

2. Is there any correlation between the individual's consistency in his successive judgments and his agreement with the average of the group?

3. Is there such a thing as general consistency, that is, does the individual whose successive judgments in this experiment show little variability judge as uniformly under other circumstances?

4. Is there any correlation between the individual's relative position in the group with respect to consistency or with respect to agreement with group average in this experiment, and the same measures in another experiment, with different material? In other words, is the individual who is variable in judging one class of material also variable in judging other material, and is his approximation to the group average in one case any index of his position for other material?

TABLE VI

Av. M.V.	Ellison	Mahon	Mordecai	Denike	Gerstein	Maschmedt	Pratt	Bishop	Schlesinger	Hart	Av.
1– 8	.59	1.08	.86	1.42	.43	1.93	.43	.43	.72	.50	.84
9–18	.77	1.68	1.05	1.84	1.21	2.48	1.41	.137	1.15	.96	1.39
18–25	1.20	2.27	1.62	3.00	1.35	1.81	1.86	1.14	1.15	1.05	1.64
25–32	1.19	1.96	1.43	1.94	1.20	.82	1.93	1.07	.86	1.35	1.37
32–39	.68	1.27	1.56	1.12	.57	.12	1.12	.34	.47	.72	.78
Av. 1–39	0.88	1.65	1.30	1.86	0.95	1.43	1.35	0.87	0.87	0.92	

TABLE VII

Observer	Size of M.V.		Correlation with Av.	
	M. V.	Pos.	*V*	Pos.
Bishop	.87	1	.296	9
Schlesinger	.87	2	.432	7
Ellison	.88	3	.320	8
Hart	.92	4	.477	6
Gerstein	.95	5	.700	1
Mordecai	1.30	6	.536	5
Pratt	1.35	7	.283	10
Maschmedt	1.43	8	.602	3
Mahon	1.65	9	.551	4
Denike	1.86	10	.664	2
	$r = -.49$ with P.E. = .16			

Table VI. gives for each individual the M.V. of the five trials for the different sections. These figures were secured by computing the final average of each card for the individual, arranging the cards in a final order of merit according to these averages and then calculating the M.V. of each position in the five trials. These M.V.'s for the eight positions in each section were then averaged to get the average M.V. for the section. The average of these gives, finally, at the bottom of each column, the average M.V. of the individual for all 39 sections. Comparison of the column shows that in six cases the M.V.'s are smaller for the upper and in four cases for the lower ranges, the average for the 10 observers being practically identical for corresponding sections, perhaps somewhat less for the lower ranges,—.84 for 1–8 as against .78 for 32–29, and 1.39 for 9–18 as against 1.37 for 25–32. So far as this difference is genuine it confirms the finding of Wells that the same individual is, on the average, more certain of the judgments in the lower than in the upper sections, differing in this respect from the judgments of the group.

Table VII. shows the consistency of each individual as measured by her average M.V., the correlation of her own average arrangement with the average arrangement of the group of 10 observers, also the position of each observer in the group with respect to both these measures. The correlation between these two main columns is −.49, indicating that a large individual variability or personal inconsistency is correlated directly with approximation to group average. This provides an interesting contrast with Wells' result for picture postals. In his experiment the individual whose personal consistency was greatest correlated most closely with the group average, and this led to the suggestion that he who knows his own mind best is likely to be the best judge of the mind of others. Downey's results pointed in the same direction. But all that the contrast shows is that the rule does not hold for the present material. As will be seen later, the process of adaptation gives to the comic situation a changing rather than a static value. As will be pointed out in the discussion of the 'chestnut' process, in the average opinion

of the group, some of the jokes change greatly in value with successive repetitions, one class rising in the relative scale, another class just as decidedly falling, while only about half of the cards approximate their original positions in the later arrangements. This means that if a given individual's judgments are to be an index of the opinion of the group, his evaluation of the waxing or waning jokes must vary correspondingly, thus giving him a high M.V. In so far as the individual's consecutive arrangements remain uniform, in just that far does he come short of being representative of his group. It is clear from this result that in all such measurements the *stability* of the material must be in some way ascertained before the results can be safely interpreted. In an unpublished study, by the writer, of the relative strength of appeals, the results of Wells and Downey are confirmed although the correlation is not high, being about +.30 with a P.E. of .08. The material for this experiment consisted of 50 descriptions of an ideal commodity, each description appealing to a different interest, instinct or effective conception. The descriptions were arranged in strict order of merit according to the persuasiveness of their appeal to the reader. Nine of the observers in the experiments on the comic took part in this experiment as well. Two sortings, one month apart, were made, and each individual's personal consistency, as measured by the correlation of her two trials, compared with her approximation to the group average, as in the present experiment. Table VIII. shows for each observer the consistency coefficient for each experiment, and also her correlation, in each, with the group average.

The correlation of the individual positions in the consistency columns

$$\left(r = 1 - \frac{6\Sigma(d^2)}{n(n^2-1)}\right)$$

is −.30. In so far as this coefficient is reliable it indicates the absence of such a thing as general consistency in the two experiments. Of course for so few cases the correlation coefficient of −.30 is not very reliable, the probable error $(.6745 \cdot 1 - r^2/\sqrt{n})$ being .20.

But there is nothing to indicate that the individual whose judgments are variable in one experiment will also be found variable in another.

TABLE VIII

Observer	Personal Consistency		Correlation with Average	
	Appeals (r)	Comic (M.V.)	Appeals	Comic
Ellison	.55	.88	.24	.32
Mahon	.13	1.65	.36	.55
Mordecai	.71	1.30	.13	.54
Denike	.78	1.86	.52	.66
Gerstein	.81	.95	.66	.70
Maschmedt	.87	1.43	.36	.60
Pratt	.74	1.35	.62	.28
Biwhop	.73	.87	.43	.30
Schlesinger	—	.87	—	.43
Hart	.8	.92	.55	.48
	r=—.30		r=+.22	

The coefficient for the correlation of approximation to the group average in the two experiments is +.22. This figure is subject to the same high probable error as is the consistency coefficient, but in so far as it is reliable it indicates a certain degree of general judicial capacity, the individual who is the best judge of her group in one case being somewhat more likely than any other to be the best judge of the same group in another case.

ADAPTATION TO THE COMIC

The writer's principal interest in carrying out the experiment lay in the study of the judgments themselves, but examination of the behavior of the cards from trial to trial discloses several clear differences. If for each trial the average value of each card be taken and the whole 39 arranged in a strict order of merit, some of the cards are found to rise in their relative position from trial to trial, others to fall as uniformly, while another large group either remain stationary or vary irregularly. This means that, aside from changing in absolute value, the jokes also change in relative merit under the influence of repetition. The group as a whole, as measured by the total value of points, we have already seen to fall in value. But some of them fall much less rapidly than others, so that cards which are originally low in the scale

mount higher and higher in relative position, while cards originally more or less high drop lower and lower. The result is that when the curves are drawn for the five successive trials, the cards differentiate themselves into a waxing, a waning and a static group. Table I. has already shown the position of each card in each trial and the following plates show the behavior of the waxing and waning groups, the heavy line in each representing the average of the total minor group. The behavior of these cards can also be calculated by comparing their individual position in the 10 piles into which they were each time sorted, thus using their approximate absolute value instead of their relative position. When this is done the curves of Plate III. are secured, in which are shown the 'absolute' change in value of the whole group and of the three minor groups (waxing, waning and static). If we assume that the observer's absolute 'standard of funniness' remained constant from week to week the relatively waxing group will have increased in absolute value as well as in relative position within the group, and we would then have a class of jokes which not only did not suffer adaptation but actually increased in funniness with increasing familiarity.

The reader will recall that the 39 jokes were classified by 10 observers under the 10 categories of the comic. Table IX. gives the score of the votes for each of the 39 jokes. Comparison of this table with the position of each card in the waxing, waning or static groups shows that the waxing group consists entirely of naïve jokes and of calamity jokes in which the predicament of the victim is self-induced; the waning group consists of the sharp retort, the pun or play on words, wit, caricature and the occupation joke; the static group includes jokes in which the nature of the category is not clear, as evidenced by the scattering votes of the judges, of a few naïve in which the naïveté involves or resembles the play on words, and the calamity joke in which the victim's predicament is brought about by the design or retort of a second party. This result seems to tally well with the facts of introspection and of daily observation. The artless remarks of children, the reply or slip which unintentionally yields amusement,

TABLE IX

CLASSIFICATION AND BEHAVIOR

No.	Carica-ture	Pun or Play on Words	Dialect	Wit	Calamity	Nâïve	Sharp Retort	Misplaced Modifier	Occupa-tion	Vulgarity, etc.	Behavior
1		7				2	1				Wane.
2	1	4		1		4					Stat.
3		2			5	2				1	Wax.
4		2			1		7				Wane.
5	1			4	1	3			1		Stat.
6	1			1		5	1		2		Wax.
7								10			Wane.
8				1		4			5		Stat.
9		1		4		1			4		Wane.
10	1	1			2	6					Wax.
11	3	1	1			3			2		Stat.
12		1		2		3	4				Stat.
13					8	1				1	Wax.
14		7	3								Stat.
15		5				5					Stat.
16				1		7			2		Wax.
17		2	1		6	1					Stat.
18		4				5			1		Wane.
19				2		8					Wax.
20							10				Wane.
21				3			7				Stat.
22		4	1	1	1	2					Stat.
23				1			8			1	Stat.
24						8	2				Stat.
25	5		1	2		1			1		Stat.
26						10					Wax.
27		2				7				1	Wax.
28	2				3	4		1			Stat.
29	1			1		8					Stat.
30				6	1		2			1	Wane.
31						10					Stat.
32						10					Wax.
33	3		2			5					Wax.
34						10					Wax.
35		9		1							Wane.
36				2	3		5				Stat.
37		8				1				1	Stat.
38	1			2	1	3	2			1	Wax.
39		10									Stat.

the harmless predicament in which the weakness, stupidity, absent-mindedness or excitement of men involve them, the Jack and Jill episodes of street and drawing room, the sudden

PLATE I

The Waxing Group

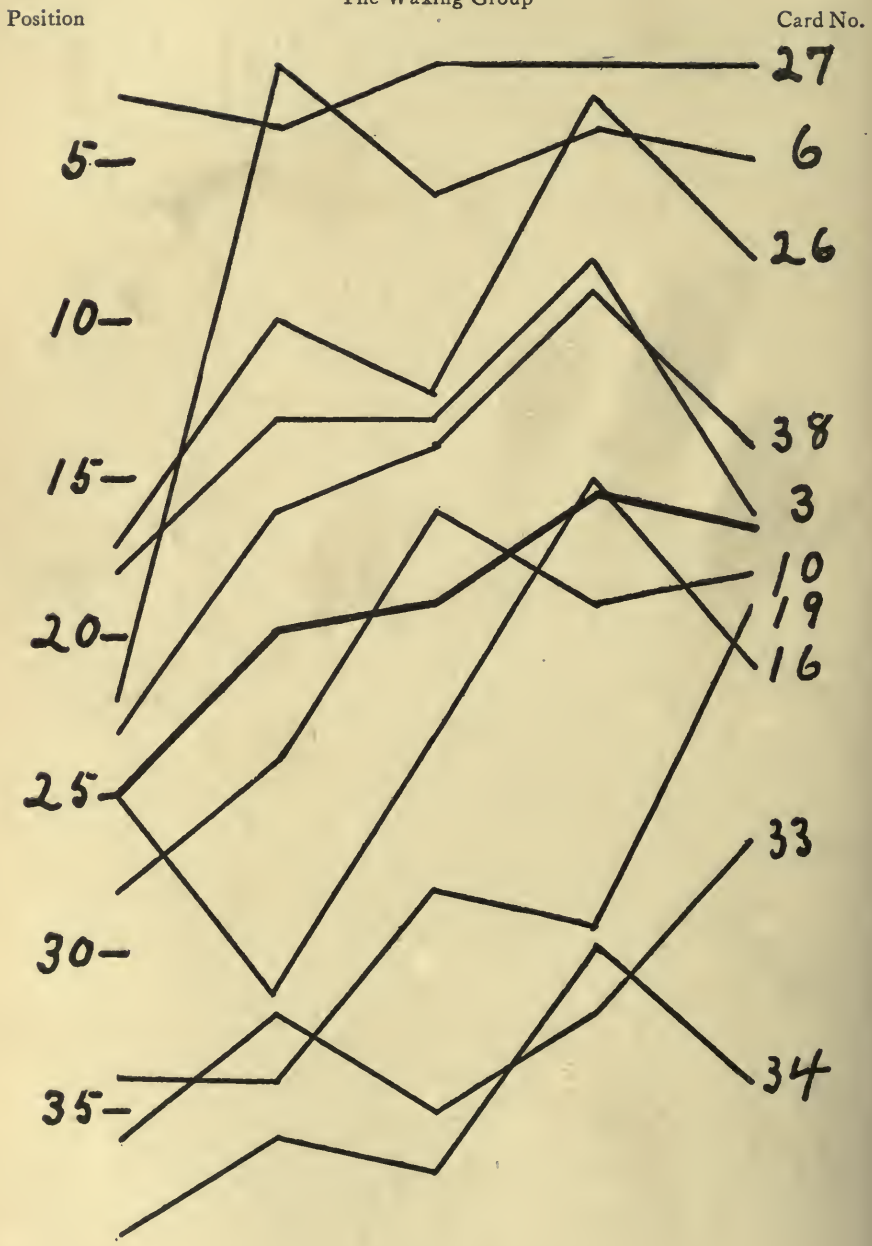


PLATE II
The Waning Group

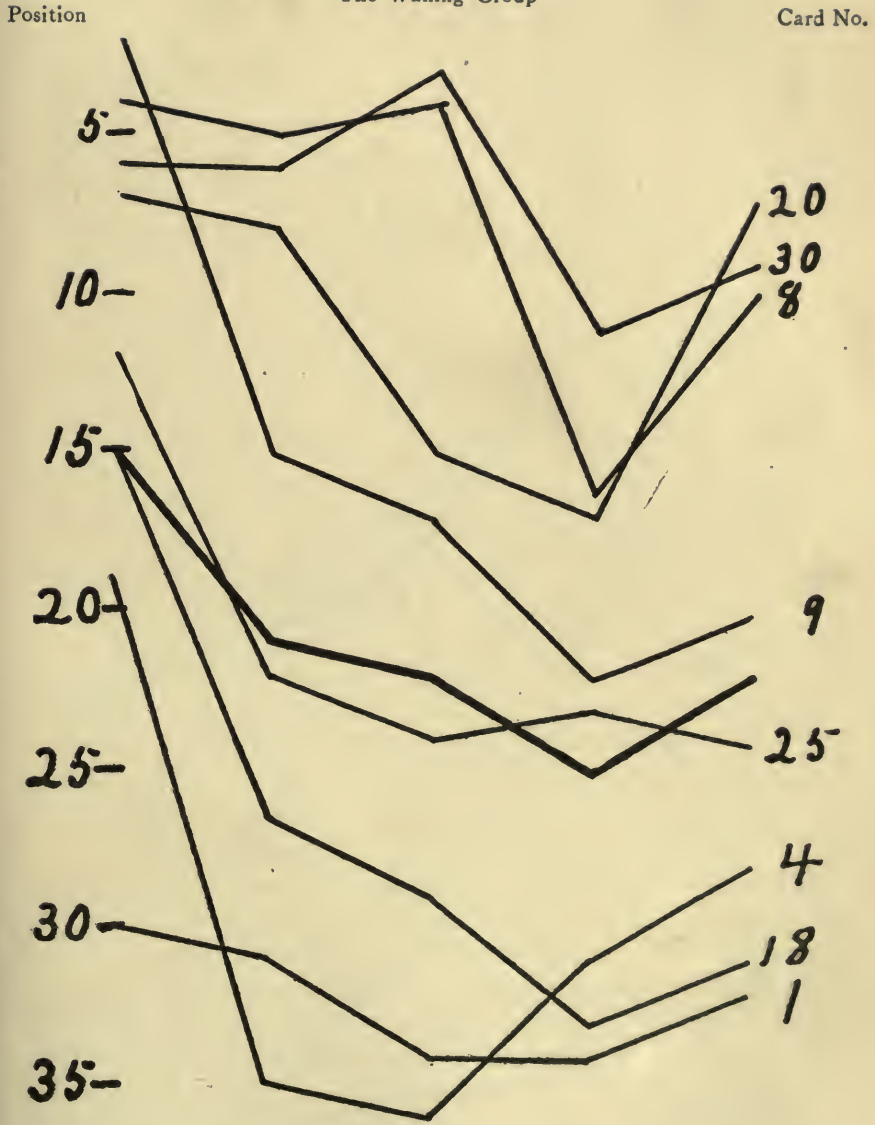
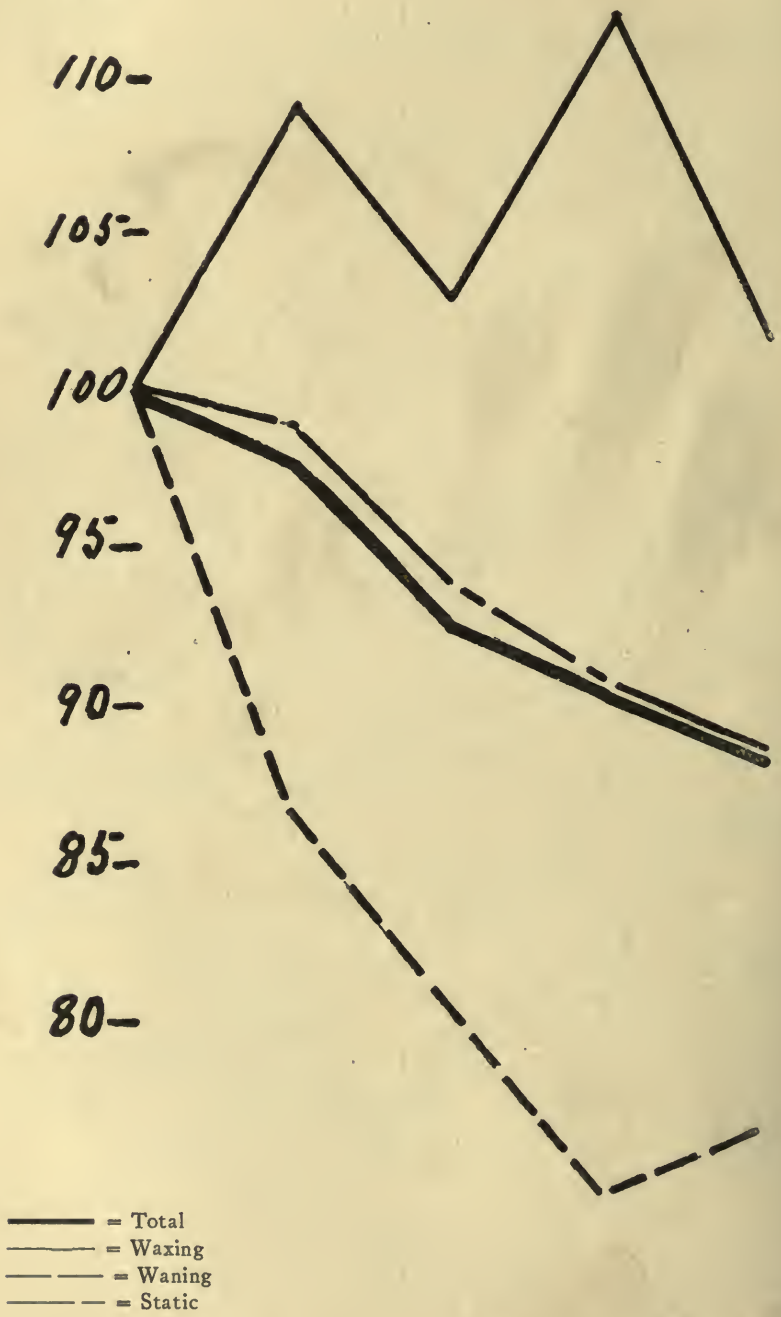


PLATE III

Change in "absolute" value in terms of per cent of original value.



revelation of unguarded human nature, etc., do not soon lose their laughter-provoking power. The whole performance is objective, does not involve any deception or subordination on our own part, and even anticipation does not rob the event of its comicality. But the pun, the witty stroke, the sharp retort, jokes whose original success depended on the degree to which they misled, deceived or tricked our own mental processes of judgment or expectation will not so well endure repetition because our subjective deception is no longer forthcoming. Anticipation and repetition cause such jests to wane rapidly, just as the present experiment indicates.

Now the static joke is likely to be one which involves both this subjective and objective relation,—thus if the calamity is self-imposed by the victim, the success of the stroke depends not so much on any deception, surprise or incongruity in the observer's mind as on the character of the spectacle itself, and the joke waxes. Whereas if the calamity is induced by the design of another it is likely to depend for its success on our own surprised reaction to a sharp retort or an unexpected verbal relation. Here then two tendencies are at work,—the calamity element which waxes, and the retort element which wanes. The net result is that the joke falls into the static minor group, neither rising nor falling in relative position, at least doing neither with regularity. Chance remarks of the observers as they went through the sortings from week to week clearly confirmed the above results, but no complete record was kept of these statements nor of the observer's introspections.

Another interesting point in the behavior of the cards is indicated in the general rise of the waning and fall of the waxing cards on the last day. This tendency to reversal seems to have been caused by the fact that, for the encouragement of the observers, who had by this time come to be somewhat bored by the performance, each was told that this was to be the last time. The effect of this remark seems to have been to increase the value of the poor jokes, so that, although the pack as a whole still dropped in value, the waning group fell more slowly than before. The final effect was that the

waning group seemed to become relatively better and the waxing relatively poorer.

The writer does not care to attempt in the present paper a thoroughgoing application of the data here reported to the theory of the comic, though such an analysis would not be at all out of place with such clear-cut results. Schauer[1] has recently attempted to show that all the forms of the comic rest on a primitive play impulse,—the delight in banter, play-fighting, 'neckerei,' and that what causes the laugh is not the 'incongruity,' 'unexpectedness' nor 'suddenly relieved tension' of the experience nor any 'feeling of superiority' in the observer, but solely the success of the trick as a play activity, the comic being here, as Miss Smith[2] has already suggested, only one form of the successful experience. Schauer then divides the comic into two types,—the objective-comic, in which the trick is perpetrated on another by natural forces, by a third person, or by the victim's own blunders, and the subjective-comic, in which we are ourselves the object of the trick and feel ourselves led on, duped or disconcerted.

The results of the present experiment accord nicely with Schauer's analysis, the waxing group corresponding to his objective-comic, and the waning group to his subjective-comic, while the static group contains both elements, either for the same or for different observers. It appears, furthermore, that the same situation, such as a stage fright, a social *faux pas* or a financial embarrassment may be originally decidedly waning in quality, but as the circumstance becomes an affair of memory and the victim is increasingly able to view his own previous predicament from the spectator's point of view, the 'oblivescence of the disagreeable' gets in its work and the experience passes from the waning into the waxing category.

[1] Schauer, O. Ueber das Wesen der Komik. *Archiv. f. d. Ges. Psychol.*, 18, 1910, p. 410.

[2] Smith, W. Comedy and the Comic Experience, PSYCHOL. BULLETIN, March, 1910, p. 84.

A LIST OF THE PUBLISHED WRITINGS OF WILLIAM JAMES[1]

1868

Unsigned review of Ch. Darwin's *The Variation of Animals and Plants under Domestication*. *Atlantic Monthly*, 1868, 22, 122–124.

1872

Unsigned review of John Morley's *Voltaire*. *Atlantic Monthly*, 1872, 30, 624–625.

1873

Unsigned review of Ray's *Contribution to Mental Pathology*. *Atlantic Monthly*, 1873, 31, 748–750.

1874

Unsigned review of *The Fifth Annual Report of the State Board of Health of Massachusetts*. *Atlantic Monthly*, 1874, 34, 234.

Unsigned review of Maudsley's *Responsibility in Mental Disease*. *Ibid.*, 1874, 34, 364–365.

Unsigned review of Carpenter's *Principles of Mental Physiology*. *Ibid.*, 1874, 34, 495.

Professor Jeffries Wyman [published anonymously]. *Harvard Advocate*, 1874, 18, 8–9.

Unsigned review of Blood's *Anæsthetic Revelations*. *Atlantic Monthly*, 1874, 34, 627–629.

1875

Unsigned review of W. S. Jevons's *The Principles of Science*. *Atlantic Monthly*, 1875, 35, 500–501.

Unsigned review of G. H. Lewes's *Problems of Life and Mind*. *Atlantic Monthly*, 1875, 36, 361–363.

1878

Remarks on Spencer's Definition of Mind as Correspondence. *Jour. of Speculative Phil.*, 1878, 12, 1–18.

Quelques Considérations sur la méthode subjective. *Critique Philosophique*, 1878, 6me année 2, 407–413.

Unsigned review of R. L. Dugdale's *The Jukes: A Study in Crime, Pauperism, Disease, and Heredity*. *Atlantic Monthly*, 1878, 41, 405.

Brute and Human Intellect. *Jour. of Speculative Phil.*, 1878, 12, 236–276.

1879

Are We Automata? *Mind*, 1879, 4, 1–22.

The Spatial Quale. *Jour. of Speculative Phil.*, 1879, 13, 64–87.

The Sentiment of Rationality. *Mind*, 1879, 4, 317–346.

[1] Information as to any omissions or inaccuracies in this list will be gratefully received if sent to Henry James, Jr., 95 Irving Street, Cambridge, Mass., or to Edwin B. Holt, Harvard University, Cambridge, Mass.

Le Sentiment de la rationalité. *Critique Philosophique*, 1879, 8me année, 2, 72–80; 81–89; 113–118; 129–136.

Unsigned review of W. R. Clifford's *Lectures and Essays*. *Nation* (N. Y.), 1879, 29, 312–313.

1880

The Association of Ideas. *Popular Sci. Monthly*, 1880, 16, 577–593.

Great Men, Great Thoughts, and the Environment. *Atlantic Monthly*, 1880, 46, 441–459.

The Feeling of Effort. Anniversary Memoirs of the Boston Society of Natural History. Boston: published by the Society, 1880. Pp. 32.

Le Sentiment de l'effort. *Critique Philosophique*, 1880, 9me année, 2, 123–128; 129–135; 145–148; 200–208; 220–224; 225–231; 289–291.

1881

Les grands Hommes, les grandes pensées, et le milieu. *Critique Philosophique*, 1881, 9me année, 2, 396–400; 407–415: et (1881) 10me année, 1, 1–14.

Reflex Action and Theism. *Unitarian Rev.*, 1881, 16, 389–416.

1882

Action réflexe et théisme. *Critique Philosophique*, 1882, 10me année, 2, 385–391; 401–410: et (1882) 11me année, 1, 5–13.

On Some Hegelisms. *Mind*, 1882, 7, 186–208.

Rationality, Activity and Faith. *Princeton Rev.*, 1882, July.

Rationalité, activité et foi. *Critique Philosophique*, 1882, 11me année, 2, 129–140; 145–154; 161–166.

The Sense of Dizziness in Deaf Mutes. *Amer. Jour. of Otology*, 1882, October.

1884

On Some Omissions of Introspective Psychology. *Mind*, 1884, 9, 1–26.

What Is an Emotion? *Ibid.*, 1884, 9, 188–205.

Absolutism and Empiricism. *Ibid.*, 1884, 9, 281–286.

The Dilemma of Determinism. *Unitarian Rev.*, 1884, 22, 193–224.

Le Dilemme du déterminisme. *Critique Philosophique*, 1884, 13me année, 2, 273–280; 305–312: et (1885) 13me année, 2, 353–362.

The Literary Remains of the Late Henry James. Edited with an Introduction, by William James. Boston: Houghton and Mifflin. 12mo, pp. 471.

1885

On the Function of Cognition. *Mind*, 1885, 10, 27–44.

Unsigned review of J. Royce's *The Religious Aspect of Philosophy*. *Atlantic Monthly*, 1885, 55, 840–843.

1886

[and G. M. Carnochan] Report of the Committee on Hypnotism. *Proc. of the Amer. Soc. for Psychical Research*, 1886, 1, 95–102.

Report of the Committeee on Mediumistic Phenomena. *Ibid.*, 1886, 1, 102–106.

The Perception of Time. *Jour. of Speculative Phil.*, 1886, 20, 374–407.

1887

Review of E. Guerney's, F. W. H. Myers's and F. Podmore's *Phantasms of the Living*. *Science*, 1887, 9, 18–20.

The Perception of Space. *Mind;* 1887, 12, 1–30; 183–211; 321–353; 516–548.
The Laws of Habit. *Popular Sci. Monthly*, 1887, 30, 433–451.
What is an Instinct? *Scribner's Mag.*, 1887, 1, 355–365.
A Suggestion for the Prevention of Sea-Sickness. *Boston Med. and Surg. Jour.*, 1887, 116, 490–491.
Some Human Instincts. *Popular Sci. Monthly*, 1887, 31, 160–170; 666–681.
Reaction-Time in the Hypnotic Trance. *Proc. of the Amer. Soc. for Psychical Research*, 1887, 1, 246–248.
The Consciousness of Lost Limbs. *Ibid.*, 1887, 1, 249–259.
The Foundations of Ethics. By John Edward Maude. Edited with a Preface, by William James. New York: Holt, 1887. 12mo, pp. iv+220.

1888

What the Will Effects. *Scribner's Mag.*, 1888, 3, 240–250.
Ce que fait la volonté. *Critique Philosophique*, 1888, nouv. série, 4me année, 1, 401–420.
Réponse de M. W. James aux remarques de M. Renouvier sur sa théorie de la volonté. *Ibid.*, 1888 nouv. série, 4me année, 2, 401–404.

1889

The Psychological Theory of Extension. *Mind*, 1889, 14, 107–109.
Note to Professor Minot's Report on the 'Diagram Tests.' *Proc. of the Amer. Soc. for Psychical Research*, 1889, 1, 317–319.
Notes on Automatic Writing. *Ibid.*, 1889, 1, 548–564.
The Psychology of Belief. *Mind*, 1889, 14, 321–352.

1890

The Hidden Self. *Scribner's Mag.*, 1890, 7, 361–373.
The Importance of Individuals. *Open Court*, 1890, 4, 2437–2440.
A Record of Observations of Certain Phenomena of Trance. Part III. *Proc. of the [Eng.] Soc. for Psychical Research*, 1890, 6, 651–659.
The Principles of Psychology. New York: Holt, 1890. 2 vols, 8vo, pp. xii+689 vi+704.

1891

The Proposed Shortening of the College Course. *Harvard Monthly*, 1891, 11, 127–137.
The Moral Philosopher and the Moral Life. *Internat. Jour. of Ethics*, 1891, 1, 330–354.

1892

What Psychical Research has Accomplished. *Forum*, 1892, 13, 727–742.
Psychology (Briefer Course). New York: Holt, 1892. 12mo, pp. xiii+478.

1894

Professor Wundt and Feelings of Innervation. PSYCHOL. REV., 1894, 1, 70–73.
The Physical Basis of Emotion. *Ibid.*, 1894, 1, 516–529.

1895

Three reviews of recent literature on 'Hallucinations and Telepathy.' *Psychol. Rev.*, 1895, 2, 65–75.
The Knowing of Things Together. *Ibid.*, 1895, 2, 105–124.
Four reviews of recent literature on 'Degeneration and Genius.' *Ibid.*, 1895, 2, 287–294.

Is Life Worth Living? *Internat. Jour. of Ethics*, 1895, **6**, 1–24.

Experimental Psychology in America. [Correspondence *in re* founding of psychological laboratories in America, including a letter from William James.] *Science*, 1895, N. S., **2**, 626–627.

Preface to F. Thilly's translation of the *Introduction to Philosophy*, by Friedrich Paulsen. New York: Holt, 1895. 8vo, pp. iii–vii.

1896

Address of the President before the Society for Psychical Research. *Proc. of the* [*Eng.*] *Soc. for Psychical Research*, 1896, **12**, 2–10. Also in *Science*, 1896, N. S., **3**, 881–888.

The Will to Believe. *New World*, 1896, **5**, 327–347.

Psychical Research. PSYCHOL. REV., 1896, **3**, 649–651.

Review of recent literature on 'Subliminal Consciousness, etc.' *Ibid.*, 1896, **3**, 682–684.

Preface to a Case of Psychic Automatism, including 'Speaking with Tongues.' *Proc. of the* [*Eng.*] *Soc. for Psychical Research*, 1896, **12**, 277–279.

Is Life Worth Living? Philadelphia: S. B. Weston, 1896. 18mo, pp. 63.

Russian edition of the *Psychology* (*Briefer Course*). Translated by I. Lapsin. St. Petersburg: P. Soikin, 1896. 8vo, pp. iv+410+23.

1897

Louis Agassiz. *Science*, 1897, N. S. **5**, 285–289. Also in *Harvard Graduate's Mag.*, 1897, **5**, 532–536. Also printed for the University, Cambridge, Mass., 1897. Pp. 12. Also anonymously in *Amer. Naturalist*, 1898, **32**, 147–157.

Oration, delivered at the unveiling of the Monument to Robert Gould Shaw, on May 31st, 1897. *The Monument to Robert Gould Shaw*. Boston: Houghton and Mifflin, 1897. Pp. 71–87. Also in Exercises at the Dedication of the Monument to Colonel Robert Gould Shaw. Boston: Municipal Printing Office, 1897. Pp. 37–53. Also in *Harvard Graduates' Mag.*, 1897, **6**, 28–37.

The Will to Believe, and Other Essays in Popular Philosophy. New York and London: Longmans, Green, 1897. 8vo, pp. xvii+332.

1898.

Consciousness under Nitrous Oxide. PSYCHOL. REV., 1898, **5**, 194–196.

Mrs. Piper, 'the Medium.' *Science*, 1898, N. S., **7**, 640–641.

Philosophical Conceptions and Practical Results. *University of California Chronicle* (Berkeley), 1898. Pp. 24.

Lehmann and Hansen on *The Telepathic Problem*. [Correspondence with E. B. Titchener.] *Science*, 1898, N. S., **8**, 956; and 1899, N. S., **9**, 654–655.

Human Immortality; *Two Supposed Objections to the Doctrine*. Boston and New York: Houghton and Mifflin, 1898. 12mo, pp. 70.

Introduction to *The Psychology of Suggestion*, by Boris Sidis. New York: Appleton, 1898. Pp. v–vii.

1899

Talks to Teachers on Psychology. *Atlantic Monthly*, 1899, **83**, 155–162; 320–329; 510–517; 617–626.

Letter *in re* Philippine Question: February 26, 1899. *Boston Evening Transcript*, 1899.

The Gospel of Relaxation. *Scribner's Mag.*, 1899, **25**, 499–507.

Telepathy Again. *Science*, 1899, N. S., **9**, 752–753.

Controversy with Prof. Titchener on Messrs. Lehmann and Hansen's *Experiments in Unconscious Whispering*. *Jour. of the* [*Eng.*] *Soc. for Psychical Research*, 1899, **9**, 113–120.

Talks to Teachers on Psychology and to Students on Some of Life's Ideals. New York and London: Holt, 1899. 8vo, pp. xi+301.

Der Wille zum Glauben. Uebersetzt von Dr. Th. Lorenz, mit einen Geleitwort von Prof. Dr. Fr. Paulsen. Stuttgart: Fr. Frommann, 1899. Pp. xx+196.

1900

La Vie, vaut-elle la peine d'être vécue? *Grande Revue*, 1900, **4**, 539–562.

Letter and translation of extracts from the diary of a French naval officer. *Springfield Daily Republican*, June 4, 1900.

Letter and translation of extracts from Prof. Blumentritt's book on the Philippines. *Ibid.*, July 2, 1900.

Principî di Psicologia. Traduzione italiana, con aggiunte e note del dott. G. C. Ferrari, diretta e riveduta dal prof. A. Tamburini. Milano: Società Editrice Libraria, 1900–1901. 8vo, pp. 927.

Japanese edition of the *Psychology* (*Briefer Course*). Translated by Dr. Motora Ujiro and Fukuki Tomokichi. Tokyo: Dobun-kan, 1900. 8vo, pp. 732.

Psychologie und Erziehung. Ansprachen an Lehrer. Aus dem Englischen von Dr. Friedrich Kiesow. Leipzig, Engelmann, 1900. Pp. x+150.

1901

Frederic Myers's Service to Psychology. *Proc. of the* [*Eng.*] *Soc. for Psychical Research*, 1901, **17**, 13–23. Also in *Popular Sci. Monthly*, 1901, **59**, 380–389.

Russian edition of *Is Life Worth Living?* Translated by N. N. S. Second edition. Moscow: Kushnerev, 1901. 8vo, pp. 39.

Russian edition of *Human Immortality*. Translated by O. A. S. Moscow: Kushnerev, 1901. 8vo, pp. 56.

Russian edition of the *Psychology* (*Briefer Course*). Translated by I. Lapsin. Third edition. St. Petersburg: Bezobrazov, 1901. 8vo, pp. 419.

The Varieties of Religious Experience: a Study in Human Nature. [Gifford Lectures.] New York and London; Longmans, Green, 1902. 8vo, pp. xii+534.

Gli Ideali della Vita. Traduzione italiana del dott. G. C. Ferrari. Torino: Fratelli Bocca, 1902. Pp. 316.

La Théorie de l'émotion, traduit de l'anglais. Introduction par le Dr. Georges Dumas. (Bibliothèque de Philosophie Contemporaine.) Paris: F. Alcan, 1902. 12mo, pp. 168.

Russian edition of *Talks to Teachers on Psychology*. Translated by A. Trombach. Edited by S. Skirmunt. Moscow: Kushnerev, 1902. 8vo, pp. 200.

1903

The Ph.D. Octopus. *Harvard Monthly*, 1903, **36**, 1–9.

Address at the Centenary of the Birth of Ralph Waldo Emerson, as observed in Concord, May 25, 1903. Printed at the Riverside Press for the Social Circle in Concord, June, 1903. 8vo, pp. 67–77.

Review of F. W. H. Myers's *Human Personality and its Survival of Bodily Death*. *Proc. of the* [*Eng.*] *Soc. for Psychical Research*, 1903, **18**, 22–33.

Du Rôle de la religion dans la vie humaine. *Annales de Philosophie Chrétienne*, 1903, 3me série, **2**, 557–564.

The True Harvard. *Harvard Graduates' Mag.*, 1903, **12**, 5–8.

Remarks on the Dedication of the Germanic Museum, Cambridge, Mass., November 10, 1903. *Harvard Illustrated Mag.*, 1903, **5**, 48–50. Also in *Harvard Bulletin*, 1903 **6**, 7.

Address *in re* Philippine Question at the Fifth Annual Meeting of the New England Anti-Imperialist League, November 28, 1903. Published by the New England Anti-Imperialist League, 1903. Pp. 21–26. Also in *New York Evening Post*, Dec. 3, 1903.

Herbert Spencer Dead. *New York Evening Post*, 1903, Dec. 8. Also as, Herbert Spencer. [Abbreviated from the signed article above, but now published anonymously.] *Nation* (*N. Y.*), 1903, **77**, 460–461.

1904

Laura Bridgman. *Atlantic Monthly*, 1904, **93**, 95–98.

The Chicago School. PSYCHOL. BULL., 1904, **1**, 1–5.

A Case of Automatic Drawing. *Popular Sci. Monthly*, 1904, **64**, 195–201.

Unsigned review of F. C. S. Schiller's *Humanism*. *Nation* (N. Y.), 1904, **78**, 175–176.

Letter on the Phillipine Policy. *Boston Transcript*, 1904, May 2.

Francis Boott. *Harvard Monthly*, 1904, **38**, 125–128. Also published anonymously in *Nation* (N. Y.), 1904, **78**, 192–193.

Herbert Spencer. *Atlantic Monthly*, 1904, **94**, 99–108.

Does 'Consciousness' Exist? *Jour. of Phil., Psychol. and Sci. Methods*, 1904, **1**, 477–491.

A World of Pure Experience. *Ibid.*, 1904, **1**, 533–543; 561–570.

Humanism and Truth. *Mind*, 1904, N. S., **13**, 457–475.

Remarks at the Peace Banquet. Official Report of the Thirteenth Universal Peace Congress. Boston: Peace Congress Committee, 1904. Pp. 266–269. Also in *Atlantic Monthly*, 1904, **94**, 845–847.

The Pragmatic Method. *Jour. of Phil., Psychol. and Sci. Methods*, 1904, **1**, 673–687.

Le varie Forme della Coscienza religiosa. Traduzione italiana di G. C. Ferrari e M. Calderoni. Prefazione di Roberto Ardigò. Torino: Fratelli Bocca, 1904. 8vo, pp. xv+467.

Russian edition of *The Will to Believe*. Translated by S. I. Tsereteli. Edited by M. V. Pirozhkov. St. Petersburg: Stasyulevitch, 1904. 8vo, pp. 390.

Russian edition of *Talks to Teachers on Psychology*. Translated by A. Trombach. Edited by S. Skirmunt. Second edition. Moscow: Kushnerev, 1904. 8vo, pp. 186.

1905

The Experience of Activity. PSYCHOL. REV., 1905, **12**, 1–17.

The Thing and Its Relations. *Jour. of Phil., Psychol., and Sci. Methods*, 1905, **2**, 29–41.

The Essence of Humanism. *Ibid.*, 1905, **2**, 113–118.

How Two Minds Can Know One Thing. *Ibid.*, 1905, **2**, 176–181.

Humanism and Truth Once More. *Mind*, 1905, N. S., **14**, 190–198.

Is Radical Empiricism Solipsistic? *Jour. of Phil., Psychol. and Sci. Methods*, 1905, **2**, 235–238.

The Place of Affectional Facts in a World of Pure Experience. *Ibid.*, 1905, **2**, 281–287.

A Knight-Errant of the Intellectual Life: Thomas Davidson. *McClure's Mag.*, 1905, **25**, 3–11.

La Notion de conscience. *Archives de Psychol.*, 1905, 5, 1–12.
La Religion comme fait psychologique. *Revue de Philos.*, 1905, 7, 5–20.
La concezione della coscienza-Leonardo, 1905, 3, 77–82.
Introduction to *The Elements of Psychology*, by Edward L. Thorndike. New York: A. G. Seiler, 1905. Pp. v–vii.
L'Expérience religieuse, essai de psychologie descriptive. Traduit par Frank Abauzit. Préface d'Émile Boutroux. Paris: F. Alcan, 1905. 8vo, pp. xxiv+449.
Sjælelære, Foredrag for Lærere og Lærerinder. [Translated by Einar Aas.] Stavanger: J. Dreyer, 1904. Pp. viii+145.
Ódaudleiki Mannsins. [*Human Immortality*, translated by G. Finnbogason.] Rey, kjavík: G. Gamalíelsson, 1905. Pp. 94.

1906

Le Pragmatisme. *Revue de Philos.*, 1906, 8, 463–484.
Stanford's Ideal Destiny. Leland Stanford Junior University Publications, Trustees' Series, 1906, no. 14, pp. 5–8. Also in *Science*, 1906, N. S., 23, 801–804.
On Some Mental Effects of the Earthquake. *Youth's Companion*, 1906, 80, 283–284.
G. Papini and the Pragmatist Movement in Italy. *Jour. of Phil., Psychol. and Sci. Methods*, 1906, 3, 337–341.
The Mad Absolute. *Ibid.*, 1906, 3, 656–657.
Mr. Pitkin's Refutation of 'Radical Empiricism.' *Ibid.*, 1906, 3, 712.
Preface to *The Problems of Philosophy*, by Harald Höffding, translated by G. M. Fisher. New York: Macmillan, 1906. Pp. v–xiv.
Gli Ideali della Vita. Traduzione italiana del dott. G. C. Ferrari. Seconda edizione. Torino: Fratelli Bocca, 1906. 8vo, pp. xx+300.
Religiöse Erfaringer. [Translated by Edw. Lehmann and Christine Mönster, with an Introduction by Prof. Harald Höffding.] Köbenhavn: V. Pio, 1906. Pp. v+386
Den Religiösa Erfarenheten. [Translated by Ivar Norberg.] Stockholm: P. A. Norstedt och Söner, 1906. Pp. vi+481.

1907

The Energies of Men. *Phil. Rev.*, 1907, 16, 1–20. Also in *Science*, 1907, N. S., 25. 321–332.
The Powers of Men. [Abbreviated and popular form of 'The Energies of Men.'] *American Mag.*, Oct. 1907 (no vol. no.), 57–65.
Les Énergies humaines. *Revue de Philos.*, 1907, 10, 317–339.
A Reply to Mr. Pitkin. *Jour. of Phil., Psychol. and Sci. Methods*, 1907, 4, 105–106.
Pragmatism's Conception of Truth. *Ibid.*, 1907, 4, 141–155.
A Defence of Pragmatism. *Popular Sci. Monthly*, 1907, 70, 193–206.
[and John E. Russell] Controversy about Truth. *Jour. of Phil., Psychol., and Sci. Methods*, 1907, 4, 289–296.
A Word More about Truth. *Ibid.*, 1907, 4, 396–406.
Professor Pratt on Truth. *Ibid.*, 1907, 4, 464–467.
The Absolute and the Strenuous Life. *Ibid.*, 1907, 4, 546–548.
A Case of Clairvoyance. *Proc. of the Amer. Soc. for Psychical Research*, 1907. 1, part 2, 221–236.
Pragmatism: a New Name for Some Old Ways of Thinking. New York and London: Longmans, Green, 1907. 8vo, pp. xiii+309.
Fases del Sentimiento Religioso. Traducción directa del Inglés por Miguel Domenge

Mir. Barcelona: Carbonell y Esteva, 1907. Tomo I., pp. 235; II., 229; (1908), III., 181.

Die Religiöse Erfahrung in ihrer Mannigfaltigkeit. Ins Deutsche übertragen von Georg Wobbermin. Leipzig: J. C. Hinrich, 1907. Pp. xxi+472.

1908

The Pragmatist Account of Truth and its Misunderstanders. *Phil. Rev.*, 1908, 17, 1–17.

The Social Value of the College-Bred. *McClure's Mag.*, 1908, 30, 419–422.

'Truth' *versus* 'Truthfulness.' *Jour. of Phil., Psychol. and Sci. Methods*, 1908, 5, 179–181.

The Meaning of the Word Truth. [Remarks at the Cornell meeting of the American Philosophical Association, December, 1907.] Privately printed, 1908. Pp. 4.

Il Significato del termine verità. *Rivista di Psicologia Applicata*, 1908, pp. 175–176.

Pluralism and Religion. *Hibbert Jour.*, 1908, 6, 721–728.

The Energies of Men. [Abbreviated and popular form of (1907) 'The Energies of Men.'] *Religion and Medicine.* New York, Moffat, Yard, 1908. No. 3, pp. 7–38.

Hegel and His Method. *Hibbert Jour.*, 1908, 7, 63–75.

Review of Marcel Hébert's *Le Pragmatisme et ses diverses formes anglo-américaines. Jour. of Phil., Psychol. and Sci. Methods*, 1908, 5, 689–694.

Der Pragmatismus. Ein neuer Name für alte Denkmethoden. Uebersetzt von W. Jerusalem. Leipzig: W. Klinkhardt, 1908. Pp. xiv+194.

L'Expérience religieuse, essai de psychologie descriptive. Traduit par Frank Abauzit. Préface d'Émile Boutroux. Deuxiéme édition revue et corrigée. Paris: F. Alcan, 1908. 8vo, pp. xxiv+449.

Psychologie und Erziehung. Ansprachen an Lehrer. Aus dem Englischen von Dr. Friedrich Kiesow. Zweite überarbeitete Auflage. Leipzig: Engelmann, 1908. Pp. x+134.

Människans Odödlighet. ['Human Immortality,' translated by Elin Silén.] Stockholm: H. Geber, 1908. Pp. 52.

1909.

The Doctrine of the Earth-Soul and of Beings Intermediate between Man and God. An Account of the Philosophy of G. T. Fechner. *Hibbert Jour.*, 1909, 7, 278–294.

Physical Phenomena at a Private Circle. *Jour. of the Amer. Soc. for Psychical Research*, 1909, 3, 109–114.

The Philosophy of Bergson. *Hibbert Jour.*, 1909, 7, 562–577.

On a Very Prevalent Abuse of Abstraction. *Popular Sci. Monthly*, 1909, 74, 485–493.

Report on Mrs. Piper's Hodgson-Control. *Proc. of the Amer. Soc. for Psychical Research*, 1909, 3, 470–589. Also in *Proc. of the* [*Eng.*] *Soc. for Psychical Research*, 1909, 23, 1–121.

The Confidences of a 'Psychical Researcher.' *American Mag.*, Oct. 1909 (no vol. no.), 580–589.

A Pluralistic Universe. [Hibbert Lectures.] New York and London: Longmans, Green, 1909. 8vo, pp. vi+405.

The Meaning of Truth: a Sequel to 'Pragmatism.' New York and London: Longmans, Green, 1909. 8vo, pp. xxii+298.

Principios de Psicologia. Traducción directa de Domingo Barnés. Madrid: D. Jorro, 1909. Tomo I., pp. xi+758; II, 711.

Précis de psychologie, Traduit par E. Baudin, et G. Bertier. [*Psychology* (*Briefer Course*).] Paris: Marcel Rivière, 1909. 8vo, pp. xxxvi+631.

Psychologie. Uebersetzt von Dr. Marie Dürr mit Anmerkungen von Prof. Dr. E. Dürr. [*Psychology* (*Briefer Course*).] Leipzig: Quelle und Meyer, 1909. Pp. iv+478.

Principî di Psicologia. Traduzione italiana, con aggiunte e note del dott. G. C. Ferrari, diretta e riveduta dal Prof. A. Tamburini. Terza edizione riveduta ed ampliata. Milano: Società Editrice Libraria, 1909. 8vo, pp. xxiv+957.

1910

Bradley or Bergson? *Jour. of Phil., Psychol. and Sci. Methods*, 1910, 7, 29–33.

A Suggestion about Mysticism. *Ibid.*, 1910, 7, 85–92.

The Moral Equivalent of War. *Amer. Assoc. for Internat. Conciliation*, 1910, no. 27, pp. 20. Also in *McClure's Mag.*, 1910, 35, 463–468. Also in *Popular Sci. Monthly*, 1910, 27, 400–412.

A Great French Philosopher at Harvard. *Nation* (N. Y.), 1910, 90, 312–314.

A Pluralistic Mystic. *Hibbert Jour.*, 1910, 8, 739–759.

Japanese edition of *Pragmatism.* Translated by Kituzawa Sadakichi, Yoshida Kei, and Nishiyama Setzuji. Tokyo: Kodo-kan, 1910. 8vo, pp. 388.

EDITORIAL ANNOUNCEMENT

The officers of the International Congress for Psychology, 1913, consisting of the two Vice-presidents, Professors E. B. Titchener and J. McKeen Cattell, and the secretary, Professor John B. Watson, voted some time ago to ask the past presidents of the American Psychological Association to serve as Vice-presidents of the Congress. At a recent meeting in New York of the enlarged body of officers, called by Professor Cattell, details and plans for the organization of the Congress were discussed. It was decided to hold the Congress at Easter, 1913. The executive committee which now jointly represents the interests both of the Congress and of the American Psychological Association consists of the following members:

James R. Angell
W. V. D. Bingham
J. McKeen Cattell
Hugo Münsterberg
E. C. Sanford
E. B. Titchener
John B. Watson

VOL. XVIII. No. 3. May, 1911.

THE PSYCHOLOGICAL REVIEW

A WORKING HYPOTHESIS FOR INNER PSYCHOPHYSICS[1]

BY RAYMOND DODGE

Wesleyan University

I. PSYCHOPHYSICAL PARALLELISM

Notwithstanding my conviction that a modified psychophysical parallelism is the least objectionable of the current concepts by which we can express the correlation of noetically mutually irreducible systems of experience, I believe that it has been and is an unfortunate working hypothesis for psychological investigation.

In the history of inner psychophysics, psychophysical parallelism has been notably sterile. In a period of relatively rapid development of psychological and physiological experience and theory, one seeks in vain for a single advance in inner psychophysics that can be accredited legitimately to the principle of psychophysical parallelism. On the contrary, in four respects at least it is a distinct handicap.

1. Aside from its metaphysics, with which psychology is only indirectly concerned, the principle of psychophysical parallelism is inhibiting rather than stimulating.

If at the outset of psychophysical investigation we postulate a thoroughgoing parallelism between the phenomena of introspection and nervous physiology, we thereby abstract at the beginning from important questions of fact. Psychology as empirical science may not shift its scientific responsibilities by a noetical construction. The correlation of nervous processes and consciousness is phenomenal not

[1] Read in part at the meeting of the New York Branch of the American Psychological Association, February 4, 1911.

noumenal. It is a problem of experience before it can properly become a problem of theoretical implication.

Now as a matter of actual experience it is conspicuous that the two series are not parallel throughout.

The phenomena of consciousness cease to exist in the only form in which they are given to us directly, while some of the processes of nervous metabolism may be demonstrated to continue. Furthermore, there seems to be little doubt that consciousness as it is given to us directly is more intimately and directly connected with the activity of the superior cerebro-spinal ganglion than with any of the others. But if consciousness, in the only form in which we can know it directly, is correlated with specific nervous processes in specific parts of the nervous system, it is part of the business of inner psychophysics to discover the peculiar characteristics of the neural processes which appear to be necessary conditions of that consciousness which we know.

This is a plain straight-forward scientific problem. Towards its solution much remains to be done. But it is obvious that the advance will not be stimulated by the belief in a thoroughgoing parallelism.

Perhaps each segment of the spinal cord, or even each neuron or physical atom, may possess a consciousness of its own. The supposition is entirely thinkable. It is, moreover, apparently incapable of disproof. But a suppositious cord consciousness is at present utterly inaccessible to us. It is not our consciousness. It is not even comprehensible in terms of our consciousness. The hypothesis of a special kind of inaccessible consciousness to correlate with every kind of neural process is utterly indefensible as empirical science. While it may be justifiable on philosophic grounds, as a psychological hypothesis it merely confuses the issue. It would be legitimate only if we could show that the various neural processes actually possess those characteristics which in the cerebrum are correlated with consciousness as we know it.

Psychic units and psychic elements never have been, and never can be correlated with physiological units and ele-

mentary physiological processes. A psychic element always appears as the resultant of a physiological manifold. A color or tone which for consciousness is not further analyzable is physiologically still highly complex. Beginning in the sense organ and ending in the cortex there are at least three links in the chain of neural happenings. In the case of color the number of links is probably higher. But in none of the links is the process a simple one. It is impossible to start the process in a single receptive element without interaction with some of the other receptive elements of the receptive organ. In the last link it spreads to a more or less general cortical disturbance with complex interplay of excitations and inhibitions.

It may be that some simple conscious process parallels the migration of each separate ion in nervous tissue. But such conscious processes are utterly inaccessible to us. We can form no conjecture from our inner experience as to what they might be like. They belong to that large but discredited class of hypotheses, that spring, not from the facts, but from the imagination. Their main function is to support an antecedent hypothesis. They are utterly unprovable and irrefutable. They have no proper place in a science of phenomena. On the contrary it is clearly a part of the business of psychology as science to ask, what is the peculiar complication of the physiological manifold whose correlate is actual sensation.

2. Both in the *Elemente* and in the *In Sachen der Psychophysik*, Fechner promulgated the now widely accepted doctrine that the 'threshold' of outer psychophysics has a counterpart in inner psychophysics. It seems to be generally agreed that even the peculiar nervous correlate of normal consciousness may be stimulated sub-liminally. Fechner held that the sub-threshold waves have a definite configuration which represents their mutual interdependence and determines their advent into consciousness. The same view finds various expression in current psychological theory.

Through its various practical and theoretical bearings on normal and abnormal mental life, the problem of the sub-

conscious excitations has become vital. Psychology cannot ignore it. But by the same token it must deal with the facts, not with mere speculations about them. I for one must refuse to believe on any merely analogical grounds that the accessible phenomena of consciousness are related to the unconscious as quantitative variations of the same functional facts. There seems to me to be a clear organic difference. It is utterly incomprehensible to me that consciousness is any mere 'emphasis' on the unconscious. Certainly there is no conclusive evidence for the supposition in the argument by analogy from external stimulation. No one really holds, as far as I am aware, that consciousness as such is directly correlated with a quantitative change in the stimulation of a sense organ. The nervous effect of supra-threshold stimulation is probably qualitatively different from that of sub-threshold stimulation. There is abundant analogical evidence in the response of living tissue to various chemical, electrical, and mechanical stimuli, that quantitative changes in the stimuli produce qualitative changes in the reactions, even to a 'reversal' of their 'sense.' There is direct physiological evidence to the point in the regular close relationship of relative fatigue, inhibition, and the refractory period, with normal reaction, 'Treppe' and 'Scheinbare Bahnung.' There is direct psychophysical evidence in the qualititave changes of sensation and feeling through quantitative changes in stimulation.

But even if the incomprehensible could be granted as demonstrated fact, psychophysical parallelism would still need limitation and modification before it would fit the phenomena of inner psychophysics. Our consciousness is obviously not correlated with the sub-threshold intensity of action of the nervous system, but only with a nervous activity of definite, *i. e.*, of supra-threshold intensity. We must face the phenomenal problems: What really happens when the threshold is reached? Why do supra-threshold intensities alone have conscious correlates in the only form in which we can know them directly? These questions, however, are matters for scientific analysis, not for speculation. And the analysis must be free from speculative prejudice.

3. Further evidence for the inadequacy of parallelism as a working hypothesis for inner psychophysics is the fact that it breaks down when we really use it, and leads to absurdity. We are utterly unable to reason successfully either from known nervous facts to consciousness, or from consciousness to its nervous correlates. Phenomenal parallelism as a working hypothesis for inner psychophysics assumes too much. It is equally embarrassed by the question where in the scale of organic existence a consciousness comparable to our own begins, as well as by the question concerning the specific conditions of the only consciousness which we can know directly.

Wherever psychology has ventured to express itself in supposedly parallel physiological concepts it has made a lamentable failure. At the present time at least we cannot use the terms quantity, force, work, fatigue, inhibition, excitation or facilitation with any proper physiological connotation in psychology. For example, we know nothing at all what a fatigue of consciousness might mean. While a consciousness of fatigue is a very different matter from physiological fatigue. Conversely animal physiology is just now seriously trying to rid itself of anthropomorphism in its concepts and in its terminology.

In short the hypothesis of psychophysical parallelism is not only sterile as a working hypothesis but it must be tinkered before it will fit the facts. It cannot be trusted anywhere, and is consequently altogether unfitted for the functions of a working hypothesis.

4. Finally, parallelism in inner psychophysics is really at heart a confession of scientific impotence. DuBois Reymond's 'Ignorabimus' may be the final word. But it was never a satisfactory starting point for developing science. It may be that 'astronomical knowledge' of neural processes will forever fail to discover consciousness. Meantime, however, it is pertinent and legitimate to ask what characteristics of the nervous processes are essential to our normal consciousness as it is immediately given to us in introspection.

The task of every science is to describe the sum total

of the conditions of the various phenomena that it investigates. Psychology has no especial privilege if it would be a science, even though the task reaches beyond our present strength.

Some of the conditions of the various phenomena of consciousness seem to be given in introspection. In no case are they completely given. Consciousness is always something more than the sum of the factors that we can introspectively analyze out of it. Wundt's principle of creative synthesis[1] must be admitted even in a descriptive psychology.[2] It is true of the simplest combinations of sensations. It is still more conspicuously true of meaning and personality.

The conditions of consciousness itself can never be accessible to direct introspection, since as conditions of consciousness they must also be conditions of introspection. Obviously direct analysis can reach only so far as introspection reaches. If in spite of the paralyzing dogma that consciousness is unanalyzable and undefinable, we persist in asking the scientific question concerning the conditions of consciousness that makes introspection possible, we must use some other technique.

Similarly, analysis of the neural process will doubtless never reach beyond itself. Certainly physiological analysis by itself will never reach the psychical, since our human consciousness is correlated not with elemental metabolic processes but with a specific organic complication of metabolic processes in a specific organ.

From both the physiological and the psychological standpoints the problem of consciousness appears as a synthetic one. We must ultimately discover what the organic complications involved in consciousness actually are. It will do us little good to catalogue the elements of which it is composed.

It is the business of a profitable working hypothesis to point out a direction of investigation that looks promising. Under the present circumstances it seems to me that the first step may well be to make some reasonable estimate of the

[1] *Phil. Stud.*, X., 1–124.

[2] Ebbinghaus, *Zeit. f. P. u. P. der S.*, IX., 161–205.

kind of organic integration that would approximate consciousness as we know it.

II. The Principle of Apperceptive Integration

While it is probable that the integrative principle of consciousness can never be separated out of consciousness by introspective analysis, since in all consciousness it is presupposed not as a factor but as consciousness itself; nevertheless, if we are to discover any clue to the integrative principle it must be from consciousness. We may reasonably expect that it will be reflected in the fundamental organizations within consciousness that are accessible to us.

If we should ask what has generally appealed to scientific psychologists as the most essential characteristics of consciousness, memory and association would probably be the favorites by a large margin. Without either, consciousness as we know it could not exist. Modifications of either by accident or disease effect grave changes in the soul life, not in its stuff merely but in its organic character.

In what seems to most psychologists an extreme and untenable position, Loeb holds that associative memory is consciousness. The psychological offence in Loeb's formulation consists in an apparent petitio principii. Psychology knows no association except the association that may occur between facts that are already conscious. Similarly in memory, what is remembered must have previously been a conscious fact. But it gives no real clue to the nature of consciousness to say that consciousness is the revival and association of conscious facts. We are bound to ask the further question: What constituted consciousness in the original of the associated and remembered fact?

I suppose that Loeb's answer to the difficulty would be to insist that both memory and association are wide embracing biological concepts. That memory is a function of all organic matter and that association is equally a characteristic of all living tissue. But in this sense, *i. e.*, the only sense in which Loeb's principle ceases to be a petitio principii, it ceases to be strictly true. If association and memory are general

biological principles, we must demand the peculiar characteristics of the associative memory that constitutes our consciousness. These characteristics it seems to me are given in the type of associative reproduction which for want of a better name we call apperception.

In using the word apperception we must make it clear that we are not using it in the restricted sense of Wundtian theory but rather in the more general sense indicated in Erdmann's apparently little known but eminently sound description of the facts.[1] By apperception then we mean that complex reproductive process that occurs in every perception of developed consciousness, in which the present stimulus immediately arouses and fuses with the residua of similar past stimulation, and mediately arouses and is interwoven with the residua of past experiences which are associated with it.

But in order to use even Erdmann's formula as the schema for all conscious integration we must abstract from the specific function in whose service the apperceptive process is here exploited, and from those characteristics of the apperceptive process which relate only to direct sense perception. Our generalized principle might be restated as follows: Apperceptive integration is a general name for a complex reproductive process which occurs in every moment of developed consciousness, in which the present nervous excitation in so far as it comes to consciousness at all, arouses immediately the residua of previous similar excitations and fuses with them; while it also arouses mediately the residua of previous excitations originally connected with it.

The question of the validity of our working hypothesis is a triple one: Could such an integrative principle give us the organization of consciousness as we know it? Does it correspond with the psychological facts at our command? Does it correspond with our physiological data?

III. Hypothetical Questions in Synthetic Psychology

If we postulate a living tissue of such character: (1) that n changes in its environment (nS) tend to excite n

[1] *Vierteljahrschrift für wiss. Phil.*, Bd. X., pp. 307 and 391.

characteristically different reactive modifications in the life history of its elements and their organic interrelations, (nR); (2) that every new S tends to reproduce every previous R in definite sequences, though in various degrees of completeness; (3) so that every new R becomes a part of a relatively slowly changing system of reactive modifications, with which it thus becomes organically integrated and to which it adds its peculiarity: should we not with such a reproductive organization of its reactive modifications grant to our tissue a kind of consciousness, in which the qualities of the original nR are logical accidents, depending on the number of discrete reactive modifications of which the elements of our hypothetical tissue are capable, within the limits of their organic interrelations?

If we further postulate a circular excito-reactive process by which the directly excited factors of the system tend to reinforce the S which aroused them, and to emphasize the corresponding R, together with other Rs immediately connected with it; while it tends to inhibit less directly connected processes, within the limits of relative fatigue; would we not therewith grant our tissue a kind of selective attention?

If we further postulate a relatively slowly changing group of emphasized Rs which are regularly reproduced with every new R must we not therewith grant our tissue a kind of personality?

IV. The Psychological Problem

As is indicated by their form, our questions make no pretence of giving an elaborated psychological theory. They aim to focalize the problem of the organic structure of consciousness, and to suggest a synthetic solution of the problem.

The fundamental principle which underlies the questions, *i. e.*, that consciousness may be known as a kind of organization, is by no means new. Psychology at the very beginning of its modern period undertook responsibility for analyzing the complex forms of organization which are directly observable within consciousness. It has made substantial additions to positive knowledge, not only in the fields of perception

memory and association, but also in the more complex fields, of language and secondary identification, meaning and co-ordinated response, and the thought processes themselves. The limits of progress seem to be determined only by the precision of our problems and the development of adequate technique.

Our thesis however goes further. Not only are there discoverable forms of organization within consciousness which characterize the systematic groupings of experience into units, temporal and spatial, practical and scientific, logical ethical, and religious, etc., but we hold that consciousness itself is not essentially different from the observable phenomena of consciousness. It is conspicuous that we can never catch consciousness except in organized form. Single conscious elements never have been discovered, and never will be. They are scientific abstractions. Moreover there is abundant and excellent psychological precedent from Kant to the present, for believing that consciousness itself may be cognized as a kind of organization.

The lamented James put the matter in his striking way: "Consciousness connotes a kind of external relation, and does not denote a special stuff or way of thinking." "The peculiarity of our experiences, that they not only are but are known, which their conscious quality is invoked to explain, is better explained by their relation to one another."

The thesis is not lacking direct scientific evidence. The splendid experimental analyses of hysteria and the subconscious, of divided consciousness and multiple personality clearly indicate that any actual unity of consciousness is neither intrinsic nor necessary, but functional. Consciousness never appears as a homogeneous quantum of some specific stuff. It always involves the unification of discrete, more or less highly differentiated factors. Divided consciousness and dual personality have the common characteristic of a disturbance of the unifying process.

Similar phenomena are familiar enough in normal mental life. Adequate stimuli of many kinds are constantly affecting the sensorium. The fact that the vast majority fail to come

to consciousness is not due to any inherent characteristics of the stimuli themselves. The same stimuli under slightly different subjective conditions may give rise to conspicuous features of our experience. In the former case available material failed to be taken up into the momentary organization of cleared consciousness. Sometimes subsequent moments appropriate it. It may affect subsequent moments without ever clearing independently. There is evidence that it may leave revivable residua. Apparently much is lost forever, and never becomes a part of our organized experience. These differences of relationship between consciousness and its material are especially conspicuous in connection with the organic sensations.

Experimental evidence is conclusive that the arousal of nervous residua is no guarantee of corresponding consciousness. Specific residua may be actively stimulated and function in the process of association without clearing up in consciousness. If the actively stimulated residua at one time fail to be included in consciousness and at another time succeed, the difference must lie in something outside the residua themselves. We can figure it in terms of our theory as a variation in the completeness of the interneural processes. The stuff of consciousness was present in both cases. What was lacking in the former instance seems to be the completeness of the organization. The unconsciously aroused residua fail of being taken up into the particular moment of organization and unification in which our present consciousness consists.

Our personal consciousness is clearly a functional unity. If possible material fails to be included, in normal life it will be inhibited; in abnormal conditions it may organize in an independent system.

Unfortunately, in its attempts to differentiate the specific character of conscious organization, psychology has been far from unanimous. A considerable variety of stimulating hypotheses is found in contemporary discussions. This is no place for a criticism of these various theories, even if I had the capacity or the interest to do it impartially. Our

present business is exposition. Critical discussion can follow, if any excuse for it arises.

An important characteristic of the principle of apperceptive integration is that it makes no claim to be a principle of explanation. The particular physiological conditions of organization are left indeterminate. But if consciousness itself, as well as its various aspects can be regarded as a kind of organization, the special problems of inner psychophysics as well as its main problem are rendered more concrete and definite.

The second claim to attention is that our principle is drawn directly from fundamental mental processes which are directly or indirectly accessible to introspection.

Perception itself, and consequently introspection, shows the process in completest form in the processes of apperception and assimilation. Various aspects of the process are conspicuous in the familiar abstractions of conscious memory and association, in habit, in the conscious controls, and in conduct.

In memory and association the application of our principle is obvious.

Habit and conduct have always offered peculiar difficulties to the application of psychophysical formulæ. The degradation of consciousness in the facilitation of habit was always an offence to the motor theories of consciousness. If we once admit, however, that consciousness is a particular kind of organization of organic reactive modifications, then it is evident that a simplification of the organization, whether by the development of direct arcs within the cerebrum, or in the lower ganglia, will lose the peculiar characteristics of the conscious organization. Secondary automatic acts will participate in consciousness in the same way that the simplest reflexes, like the knee jerk and the protective wink participate. Situation and response will be perceived as phases of a single process.

In so-called voluntary activity, on the other hand, with its conscious suppressions and reinforcements, the resulting activity may, and in its highest form will, represent the

resultant of the completest practicable interplay of the individual's past, present, and future experiences. Each relatively steady control adds to the complexity of the regularly reproduced reactive modifications which make up the personality.

Conversely in organisms whose nervous systems contain only relatively simple reflex mechanisms, there is neither ground nor excuse for assuming the complex reproductive processes of consciousness. The application of our principle to the problems of animal consciousness is at least a clear cut issue.

Loeb and others make the criterion of consciousness the educability of an organism. Against this all the force of Becker's arguments against Driesch apply without the possibility of Driesch's answer. Moreover, it is confronted by empirical contradiction. Many of our human reflexes show development and change by repetition without conscious participation.

Miss Washburn suggests that the simplest consciousness consists in a series of flashes. If amœbas have minds that act in a series of flashes, where these minds may be and what they are when they are not flashing are as unexplained and inexplicable as the nature of the flash itself. On the basis of our organic principle we are bound to ask whether the amœba is of such organic complexity that a reactive modification can reproduce past reactive modifications in any definite series however primitive. I believe that the available evidence is against it.

In a single cell all internal reactive modification is at the same time external. But the actual reactions of amœba, though they show adaptation give no evidence of habitual series of differentiated reactions resulting from each new stimulation. Such systems would be a menace to its welfare, or to the welfare of any creature in which external reaction and internal reaction were identical. Only in a differentiated tissue, whose reactive modifications do not find immediate expression in action, would the reproduction of previous modifications in habitual systems be tolerable. Such a dif-

ferentiated tissue seems to be observable only in the nervous ganglia.

A cell consciousness comparable to our consciousness seems improbable. Aside from the difficulty that we can discover no suitable mechanism, the very simplicity of the life history of the single cell leaves no room for complex differentiated experiences. Even if by some hocus pocus a single cell could be artificially endowed with differentiated experiences revivable in definite chains, it would be an utterly useless burden. In the monotonous reactions of its simple life it must soon decay from disuse.

V. The Physiological Problem

From the standpoint of biology and physiology, I believe that the conception of consciousness under the schema of a specific kind of organization is just as conspicuously an outgrowth of the data. With the epoch-making developments in biological science which began in the middle of the last century, the motives for such a conception were very strong. The early biological theories, however, suffered from the general preëxperimental confusion between psychological fact and psychological hypothesis. Moreover, they often confused the issue with mechanical or materialistic interpretations.

With the discovery of the electrical currents of action in nerve and muscle, theories of nervous organization followed the analogy of the telegraph. The cerebrum corresponded to the central exchange or switch-board. Energy was supposed to enter the system in sense stimulation. Variously combined and modified it found exit as a motor impulse. Consciousness was a product of the switching process.

Traces of this conception are still found in popular thought and even in psychological text-books. But the present scientific view of the action of the nervous system is fundamentally different. Each of the millions of neurons whose various clusters make the anatomical units of the nervous system, seems to be a rather independent little body, and leads a relatively independent life. It has its own supply of energy; regulates to a considerable degree its own nutrition and

growth; reacts in its own peculiar way to the chemical and nervous stimuli to which it happens to be sensitive; and finally, it may die without necessarily involving the destruction of its neighbors. In general each neuron seems to be particularly sensitive to some particular kind of stimulus. But its action in every case seems to be relatively independent of the kind of stimulus that excites it. That appears to be determined primarily by its own specific form of energizing. Each neuron may stimulate an indefinite number of other neurons; and each may in turn be variously stimulated. The end of the chain may be gland, muscle, or nervous tissue. Whatever the final outcome, each step in the process, including the last, is determined by the character of the participant links. The result, as measured at different points of the process, may bear widely different ratios to the energy of the original stimulation. It may become negative in direct inhibition. It may reach enormous proportions by association.

Take, for example, the familiar phenomenon of the knee jerk. Considerable pressure against the tendon produces no reaction. The same energy in the form of a quick tap may develop a vigorous reaction. It is possible to adjust the stimulus so that it develops more than twenty-five times its energy in the reaction. But if the same blows are given in a series of about ten per second, the reactions entirely disappear in a relative refractory phase.[1]

Similarly, ether vibrations of barely threshold intensity, corresponding to the light reflected from a thin column of smoke in a darkened theater, may produce extensive neural excitations. It may lead to the maximum activity of the largest muscles in the body. Nervous activity is no longer regarded as the central reflection and final motor emergence of the afferent energy derived from sense stimulation. Each link in the chain is a magazine of potential energy, which reacts to adequate stimulation in its own way. Every sensory-motor reaction may thus be traced through the nervous system as a chain of stimulations and adjustments.

Somewhere in that process our consciousness arises.

[1] Dodge, *Zeitschrift für allgemeine Physiologie*, Bd. XII., pp. 53–55.

There is evidence that its locus is not in the peripheral sense organ. Though there have been recent attempts to connect it with the basal ganglia, the weight of evidence forces us to regard it as correlated with cerebral activities.

The Cartesian theory of a single point of contact between soul and body is clearly inadequate. It was long ago abandoned as a tenable hypothesis in physiology. But its effects are still felt. True there is no single place discoverable where consciousness as such is located. No circumscribed or focal lesion necessarily destroys consciousness as a whole. But it is not uncommonly held by psychologists and physiologists alike that certain kinds of consciousness are located in discrete cortical areas. Certainly there is no warrant for such an assumption in our present knowledge of the localization of function. It is indeed true that each of the several sensory projection fields has peculiar functions with relation to the rest which no other field can vicariously affect. But we cannot say that a certain kind of consciousness is here or there. If a given field is destroyed consciousness will lose some of its possible variations, but consciousness will remain. It will be a distorted, crippled consciousness, with more or less conspicuous differences from the normal. The brightness and the song will be gone. But a very human consciousness without either vision or audition is a conspicuous reality. The assumption, on the other hand, that the visual projection area contains the correlate of a visual consciousness, which might persist if these areas could be isolated, is without a shred of evidence. The anatomical provision for the intricate organization of each part with all the rest is certainly not meaningless. Its obvious lesson is confirmed by some of the most assured products of psychological analysis, which indicate the complexity of the physiological conditions of even the simplest concrete facts of consciousness.

If some of the present tendencies in physiology appear to emphasize the processes within the cell units, the physiology of the cerebrum never more clearly faced the problem of the importance of the systematic interrelations of the neurons. This is especially conspicuous in the substitution of the con-

cept of a point of maximum vulnerability for the old concept of localization, in the speech disorders. The assumption that consciousness is somehow connected with the individual cells is certainly not based on any clear notion of how the intra-neuron processes are related to consciousness. I conjecture it is rather because it is recognized that the cerebrum is an integrating organ and it is uncritically assumed that the elements must have the same characteristics as the integrated whole.

VI. The Stuff of Consciousness

The question as to the nature of the unorganized stuff of consciousness is not really a part of our present discussion. But it is bound to be asked and ultimately it is vital. Is there a peculiar soul stuff, or not? For myself I must confess that the problem is tremendously interesting, but it seems to me that this is an added motive to guard against methodological error.

Whatever the answer, as far as science is concerned, it must be in the form of a hypothesis. There will be a characteristic difference between this hypothesis and the hypothesis of our questions. The latter appeared to be not a principle of explanation, but a working hypothesis. The motive for the former is not practical as far as I can see but purely speculative. The question must be asked. It will be answered. The speculative demands of human reason must be satisfied or the completest organization of consciousness will be lacking. But the question and its answer are not directly pertinent to the main question at issue.

On the other hand, our fundamental hypothesis does effect a certain control of possible answers. It is obvious that on this basis, the stuff of consciousness cannot be sensations, feelings, memory images or any of the other direct products of introspective analysis. As far as these facts are not pure abstractions, they obviously serve as building material for the complex organizations within consciousness. They cannot be the unintegrated stuff of consciousness because they are already conscious.

It is widely held that certain moral needs demand that

the stuff of consciousness be a peculiar sort of soul stuff. The demand arises in the effort to escape the determinism of a materialistic universe. These needs certainly deserve a hearing. But it never seemed to me that the hypothesis of a special soul stuff helped matters much when we are forever forced to deal with that stuff and its combinations under the category of causality. Neither does it seem to me that the stuff of our material world-construct can be held responsible for its organization. It cannot supply the principles of its organization without being more than stuff.

One is justly suspicious of cycle and epicycle of hypotheses. There are good methodological grounds for making explanatory hypotheses as simple as possible. In the present case these grounds constitute a motive for the hypothesis that the stuff of our spatial construct, on the one hand, and of our consciousness, on the other, have no necessary and intrinsic differences. The stuff of conscious integration must be capable of that kind of organization. But I can see no intrinsic reason why the same stuff should not be capable of organization in the other series too. I can discover no other necessary peculiarity for the stuff of consciousness than the fact that it is so organized. There is no negation given or implied. As far as I can see the stuff of consciousness might enter into all sorts of different systems, as any given atom in the body is also in systematic relations with the inanimate.

In one very fundamental sense metaphysical parallelism was not affected by our critique of parallelism as a working hypothesis for inner psychophysics. It seems to me that we might well hold that the stuff of the universe shows parallel attributes. That it is at once a soul stuff and a matter stuff. But I can understand this in no other way than one might say that an atom of carbon is either organic or inorganic. As atom it is neither. In the course of history it may be both. But this sort of parallelism makes no pretense to knowledge of the one form of organization from knowledge of the other form. On this basis it would be absurd to hold that the organization of the primitive stuff in consciousness

was paralleled by the organization of some more of the primitive stuff in brain tissue. The concept would be ludicrous and I believe it is ludicrous.

To sum up: My main contention is that the stuff of consciousness is a logical accident. Whatever it were, the right kind of integration would constitute a kind of consciousness. I can see no reason why any stuff in the universe may not enter into a similar kind of organization, if the proper conditions are given.

THE RELATION OF THE TIME OF A JUDGMENT TO ITS ACCURACY

BY V. A. C. HENMON
The University of Wisconsin

Present-day interest in sensory judgments of small differences apparently lies not so much in the determination of the differential threshold and the validity of Weber's law as in the analysis of the factors which condition the judgments themselves. Among the various factors involved in the judgment of liminal differences the time required or allowed for arriving at the judgment is one that has been given little consideration. No doubt the reason for this is the one recently expressed by Titchener[1] that the times of judgment of comparison, etc., "become extremely variable, and have little significance." Judgment times are obviously very variable, but it nevertheless is not improbable that their measurement may serve as a valuable means of analysis and not "merely as a convenient setting for exercises in introspection," just as the measurement of the *Trefferzeit* in experiments on memory has been of value in the study of reproduction and the force of associations. Martin and Müller[2] in their experiments with lifted weights, recorded the times of judgment without the knowledge of their subjects, by counting the metronome beats intervening between the raising of the second weight and the pronouncing of the judgment, the metronome beating at intervals of .7 sec. In summarizing the results of the time measurements, they conclude: "(1) That the time of a judgment is dependent on the psychic factors involved, *i. e.*, whether the judgment follows immediately from the absolute impression of one of two stimuli, from a comparison of the stimuli, from secondary comparisons, or from the coöperation of several such factors;

[1]Titchener, E. B., 'Text-book of Psychology,' New York, 1910, p. 447.

[2]Martin, L. J., and Müller, G. E., 'Zur Analyse der Unterschiedsempfindlichkeit,' Leipzig, 1899, pp. 196–206.

(2) with constancy in the factors producing the judgment and under constant conditions, the time of a judgment is shorter the clearer the psychic factors are; if the judgment depends upon the non-appearance of such factors as 'doubtful' and 'undecided' then the time of judgment is longer than when the judgment is determined by positive factors; (3) the time of a judgment depends on whether the psychic factors concerned are associated in an unambiguous way with a particular category of judgment; if the judgment falls in an intermediate region, *e. g.*, between 'smaller' and 'clearly smaller,' then the time is longer even than in cases when the perception of difference is less clear, but happens to coincide with a definite category of judgment; (4) the time of a judgment varies with individuals and with all the circumstances which are likely to influence the subject's degree of confidence." These measurements are admittedly rough and the authors suggest that it might be worth while in the future to undertake the more exact time measurements in psychophysical experiments where this would not interfere with the main purposes of the investigation.

Daily experience reveals great variations in the length of time taken for pronouncing judgments under different conditions and by different individuals and suggests that the time of a judgment is in some way correlated with its accuracy. There would supposedly be a considerable difference in both the character and accuracy of a judgment rendered after half a second and one given after one, two or three seconds. One might expect that, eliminating adaptation and fatigue, the judgment would grow in accuracy the longer the time taken to make it, provided the stimuli remained until the judgment was rendered. There would be a gradual maturing of the factors involved and a corresponding increase in confidence and accuracy. In many cases this does represent the course of a sensory judgment. The judgment of difference grows steadily to a point of maximal clearness. However, one notes introspectively that in many cases a prolongation of the time beyond that required for the first judgment that could be made introduces doubt and indecision. Instead of

an increase in certainty and accuracy there is an increase in uncertainty and inaccuracy. Both considerations suggest that there may be an *optimal* time for judgment, varying with individuals and with varying stimuli and conditions. If this be true then wrong judgments would include among others those given before the optimal time had been reached or after it.

Introspection reveals various alternative processes, even in the short times required to judge the lengths of the two lines when the differences are liminal: (1) Prepossession leads one to expect a line to the right or to the left and a mere glance from one line to the other gives a very quick and coercive judgment, that is given with confidence and is reliable. (2) An initial tentative judgment is gradually confirmed and there results a steady increase in confidence in its accuracy. In such cases the difference between the lines appears to grow before one's eyes and the judgment is finally made with confidence and accuracy. Such judgments are relatively quick. (3) There may be a rapid alternation of judgments beginning either with an initial judgment of equality or with a tentative judgment of difference, with a change of the judgment as each line is successively fixated or attended to. Here there are in a single experiment lasting a second or less not one but several judgments, the final judgment being a compromise, taking usually a longer time than judgments of the other types and ordinarily given with little confidence and reliability. The resolution of an initial judgment of equality sometimes takes place with a sudden expansion or telescoping of one line or the other, or again it may take place slowly. (4) The first tentative judgment is gradually or quickly followed by doubt and finally by reversal, sometimes with great confidence and reliability, but more often with little confidence and reliability. All of these instances are typical cases which suggest the possible significance of the time factor in judgment.

It was with a view of studying the relation between the times of judgments and their accuracy, as well as the determination of individual differences in judgment times, that

the present investigation was undertaken. The stimuli employed were lines, chosen because of their convenience for exposure and time measurements. The lines were 20 and 20.3 mm. long, 2 mm. in width, and printed horizontally on the cards 10 mm. apart. The length of lines, which differed by 1/66.6, was selected with a view to securing a stimulus difference that would give approximately 84 per cent. of right cases, in accordance with Fechner's suggestion that such a difference would give the most satisfactory results. The plate from which the cards were printed was prepared by an expert engraver and the lines were very accurate. The cards were exposed by means of the spring drop screen used by the writer in experiments on the time of perception as a measure of differences in sensations.[1] The exposure was continued until the reaction or judgment was made. Immediately after the reaction the shutter was closed. The average of a great many trials showed that the screen was raised approximately 300σ after the reaction. In one half of the judgments the subjects were to judge which was the longer line and in the other half the shorter. The times were recorded in the usual manner with the Hipp chronoscope, reactions being made by the right hand if the line to be judged was to the right and by the left hand if it was on the left. The distances of the subjects' eyes from the screen was 50 cm.

From each of three subjects, Miss Madge Brown (Br), of the University of Colorado, to whom I am under especial obligations for great assistance both in the conduct of the experiments and in the laborious calculations, Mr. C. L. Bluemel (Bl), also of the University of Colorado, and the writer (H), 1,000 judgments and judgment times were secured. Fifty judgments constituted a series and ordinarily one series a day was taken. The usual precautions were observed to secure uniformity in conditions, such as control of the chronoscope, time of day, order of series, uniformity in light conditions, etc. Immediately after each judgment the subjects were asked to assign the degree of confidence in the accuracy

[1] For description see writer's 'The Time of Perception as a Measure of Differences in Sensations,' *Archives of Philos., Psychol. and Scientific Methods*, Science Press, New York, 1906, p. 18.

of the judgment, *a* for 'perfectly confident,' *b* 'fairly confident,' *c* 'with little confidence' and *d* 'doubtful.' In each case the subject was asked to judge whether a line was longer or shorter hence the small number of the 'equal' and 'undecided,' cases which occurred are included under the *d* judgments.

The general summary of the results of 3,000 judgments appears in Table I., which gives the distribution of the right and wrong cases with each degree of confidence, the corresponding judgment times in σ or thousandths of a second, and the mean variations. The last line in the table gives the total number of right and wrong cases and the average time of judgment.

The accuracy of judgment is shown by the percentages of right cases, which are 82 per cent., 77.4 per cent. and 83.2 per cent. If the *d* judgments are divided equally between the right and wrong cases, according to the usual procedure, these percentages become 82.1 per cent., 73.2 per cent. and 81.6 per cent. However, the *d* judgments, at least for subjects Br and H, include cases which are not strictly doubtful or equal, but are rather judgments with a minimum of confidence.

The correspondence between confidence and accuracy is high, indicating the reliability of the introspections. There is a uniform decrease in the number of right cases with the decrease in confidence as the following percentages of right cases with each degree of confidence show:

	a	*b*	*c*	*d*
Bl.....	96.6	84.1	65.3	48.0
Br.....	95.1	94.4	83.6	61.5
H.....	100.	97.2	85.3	56.3
Av.....	97.2	91.9	78.1	55.3

The nature of the decrease in accuracy with decrease in confidence and the correlation between the subjective scale of confidence and the degree of mathematical probability is a problem which is beside the purpose of this study.[1]

[1] Cf. Fullerton and Cattell, 'Perception of Small Differences,' Philadelphia, 1892, pp. 14, 60–64, 124–127, 132–133, 142–145.

TABLE I

Degree of Confidence	Bl						Br						H					
	R.			W.			R.			W.			R.			W.		
	No.	σ	M.V.	No.	σ	M.V.	No.	σ	M.V.	No.	σ	M.V.	No.	σ	M.V.	No.	σ	M.V.
a.....	337	753.1	190	12	557.0	132	98	559.5	136	5	574.4	148	93	637.6	104	0		
b.....	318	1044.8	308	60	986.9	317	235	595.9	165	14	668.9	206	312	721.8	124	9	698.8	122
c.....	128	1311.3	356	68	1204.7	344	216	635.2	151	66	605.9	171	285	788.7	133	49	777.3	143
d.....	37	1612.0	519	40	1499.1	555	225	624.0	156	141	596.2	150	142	849.6	145	110	813.6	147
	820	992.1		180	1154.3		774	610.4		226	602.7		832	770.3		168	796.8	

The subjective scale at any rate is less 'crude and inconstant' than Titchener apparently holds it to be and the correlation is relatively high.[1] His statement that "We 'feel sure,' our expectation becomes conviction, long before we have the objective right to be anything more than very moderately expectant" is not borne out by these experiment. It is perhaps worth noting that the percentage of right *d* judgments (55 per cent.) is less than that obtained by Pierce and Jastrow (60 per cent.), by Fullerton and Cattell (60 per cent. and 65 per cent.) and by Griffing (59 per cent.). The individual differences both in the degree of confidence and in the times of judgment are discussed in a later section.

The times of judgment for all right judgments is shorter than the average for the wrong judgments, except in the case of Br, where the difference is not significant. However, in all cases but two the times for wrong judgments under each category of judgment is longer than for the right judgments. The times of wrong judgments are both absolutely and relatively more variable.

The relation between the time of judgment and degree of confidence is indicated by a uniform increase in time with a decrease in confidence, except in the case of the *c* and *d* judgments of Br. Martin and Müller[2] suggest that "if the cases in which the judgment kl or gr are given be arranged in classes according to the times of judgment, in which the lowest class would include the longest and the highest class the shortest judgment times then perhaps the distinction between kl [less] and *kl* [less (clearly)], gr [greater] and *gr* [greater (clearly)] cases might be dispensed with altogether." With small differences where the absolute impression does not enter in as a factor and where the variability in time of judgment is as great as is these experiments, this could certainly not be done. The rise of judgments to the same degree of clearness, so far as one can introspectively estimate it, may be quick or slow, even though the average times do show a definite correspondence between time and confidence.

[1] Titchener, E. B.. 'Experimental Psychology, Quantitative, Students Manual,' New York, 1905, pp. 53–55.

[2] *Op. cit.*, p. 206.

The uniformity in the increase in time is indicative of the absence of such factors as the absolute impression, slip comparisons, etc., which would be likely to betray themselves in lengthened or shortened judgment times.

The nature of the correlation between the time and accuracy of judgment is difficult to determine and indicate. Müller[1] gives the psychological conditions of *zufällige Fehlervorgänge* as "(*a*) Varying degree and direction of attention; (*b*) variation of the constituent process in the given complex by which judgment is determined; (*c*) degree and direction of expectation and imagination; (*d*) influence of chance order of preceding experiments" (quoted from Titchener, 'Experimental Psychology, Quantitative, Instructor's Manual,' p. 313). The introspective records kept in the course of these experiments suggested that among the conditions that lead to wrong judgments, the time factor is significant. The subjects noted in many cases that the judgments, which later were shown to be wrong, included a considerable number that were judged to have been made either too soon or too late. This is indicated to some extent in Table II, which gives the distribution of the right and wrong cases according to time and the percentage of cases falling within such time-groups.

Owing to the complexity of the factors that cause erroneous judgments the evidence for the existence of the two types of wrong judgments does not emerge as clearly as the introspections led us to expect. H shows a greater percentage of long erroneous judgments, Br a greater percentage of short erroneous judgments, while Bl gives slight evidence of both types. The differences in percentages are small and are obscured by a variety of other factors. A still larger number of measurements would be required to indicate the time factor decisively and in addition a series of experiments where the time taken in judgment was voluntarily controlled.

One curious constant error in judgments of the shorter line appeared in the results. All of the subjects, particularly

[1] Müller, G. E., 'Die Gesichtspunkte und die Tatsachen der psychophysischen Methodik,' Wiesbaden, 1904, § 21.

TABLE II

		100–300	300–500	500–700	700–900	900–1100	1100–1300	1300–1500	1500–1700	1700–1900	1900–2100	2100–2300	2300–2500	2500+
Bl	Right cases	1	46	185	197	135	84	64	50	26	14	10	4	4
	Wrong cases	1	11	29	23	24	29	21	24	6	4	1	4	3
	Per cent. R.	.001	.056	.225	.240	.165	.102	.079	.061	.032	.017	.012	.005	.005
	Per cent. W.	.006	.061	.161	.127	.133	.161	.116	.134	.033	.022	.006	.023	.017
Br	Right cases	2	261	302	133	52	18	5	1					
	Wrong cases		89	79	37	13	8							
	Per cent. R.	.003	.337	.390	.174	.066	.023	.006	.001					
	Per cent. W.		.394	.350	.164	.057	.035							
H	Right cases	3	36	286	350	127	25	5						
	Wrong cases		7	47	69	33	11	1						
	Per cent. R.	.004	.043	.344	.421	.152	.030	.006						
	Per cent. W.		.042	.279	.410	.197	.066	.006						
Av.	Right cases	6	343	773	680	314	127	74	51	26	14	10	4	4
	Wrong cases	1	107	155	129	70	48	22	24	6	4	1	4	3
	Per cent. R.	.001	.145	.320	.278	.128	.052	.030	.021	.011	.006	.004	.001	.002
	Per cent. W.	.006	.166	.263	.233	.129	.087	.041	.045	.011	.007	.002	.008	.006

Br and H, noted early in the experiments that judgment could be given more easily, more quickly and with greater confidence when reaction was to be made to the shorter line. The feeling that the most accurate judgments would be secured with the shorter line was very marked. Table III gives the distribution of judgments to the longer and shorter line.

The results in part confirm the introspections and in part do not. The general averages show in each case that the greater number of wrong judgments was obtained from reactions to the shorter line though the differences are significant only in the case of Br. However, the number of right *a* judgments to the shorter line is almost twice as great as to the longer line, except in the case of Bl where the difference is not marked. The results indicate in an interesting way both the accuracy and inaccuracy of introspections in the judgment of small differences.

In summary the following conclusions with reference to the relation of the time of judgment to accuracy and confidence may be noted:

1. The time of judgment increases uniformly as the degree of confidence decreases.

2. The time of wrong judgments is on the average longer than the time of right judgments, while under each category of judgment the wrong judgments are in general shorter.

3. The time of wrong judgments is more variable than that of right judgments, and there are indications of two types of wrong judgments with reference to time, those that are rendered too quickly and those that are prolonged beyond the optimal time.

A continuation of this investigation (1) where the time of exposure of stimuli was limited, (2) where the time of judgment was voluntarily shortened or prolonged, (3) and with varying differences in stimuli, should give significant results.[1]

[1] The writer had planned such an investigation, but a change of work has necessitated its indefinite postponement; hence the publication of these preliminary results.

Table III

	Bl				Br				H			
	R.		W.		R.		W.		R.		W.	
	Longer	Shorter	Longer	Shorter	Longer	Shorter	Longer	Shorter	Longer	Shorter	Longer	Shorter
a.....	160	177	2	10	33	65	1	4	33	60	0	0
b.....	178	140	29	31	126	109	6	8	149	163	2	7
c.....	62	66	40	28	128	88	27	39	155	130	18	31
d.....	14	23	15	25	114	111	66	75	81	61	62	48
Total.....	414	406	86	94	401	373	100	126	418	414	82	86

Individual Differences

Experiences in daily life show the wide range of differences in the speed of judgment as well as in accuracy and the degree of confidence with which it is made. With a view to securing data on these differences and their correlations, 500 judgments and judgment times, under the same conditions and with the same stimuli as used in the above described experiments, were obtained from each of seven subjects, five women and two men, all of whom had had practically a year's laboratory training in psychology or more. These measurements, together with those of the three subjects of the above experiments, thus give records for six women and four men. The results appear in Table IV.

What strikes one at first is the variation in the times of judgments ranging from averages for all judgments of 304σ for A to 1095σ for E. The group is too small to reveal type differences which from common observation might be expected to occur. The variation in accuracy runs from 68.6 per cent. of right cases for A to 83.2 per cent. for H, the range being almost exactly as 1:2. This is the variability noted by Cattell[1] in the judgments of the ten observers who ranked the American psychologists and which he says is "approximately the variability that I have found in normal individuals in other mental traits, accuracy of perception, time of mental processes, memory, etc." Wells[2] found a similar variability in judgments of literary merit. With reference to sex differences in rapidity of perception and sense judgments it is generally supposed that women are quicker than men. This difference does not appear in these results. Subjects A, B, C, D, E and Br are women; F, G, Bl and H are men. The shortest judgment times are by two women, A and B, and the two longest times are also by two women, D and E. In accuracy the sex differences are insignificant, the averages for the men being 77.2 per cent. and for the women 76.9 per cent., with the two highest percentages by two men, Bl and H.

[1] Cattell, J. McKeen, 'Statistics of American Psychologists,' *Am. Jour. of Psychol.*, XIV., 1903, p. 581.

[2] Wells, F. Lyman, 'A Statistical Study of Literary Merit,' *Archives of Psychology*, New York, 1907, p. 27.

Table IV

	A						B						C						D					
	Right			Wrong			Right			Wrong			Right			Wrong			Right			Wrong		
	No.	Time	M.V	No.	Time	M.V	No.	Time	M.V	No.	Time	M.V	No.	Time	M.V	No.	Time	M.V	No.	Time	M.V	No.	Time	M.V
a.	58	350	102	7	427	120	304	518	129	38	513	150	154	513	87	11	443	59	376	1031	373	76	1006	347
b.	152	299	86	18	320	95	79	599	188	62	527	133	153	657	129	18	559	122	21	1242	664	20	1215	586
c.	76	281	64	28	296	94	6	734	142	11	608	222	72	794	159	45	651	114	2	1162	334	4	963	427
d.	57	292	83	104	289	82							21	914	214	26	687	132				1	1043	
Total R. and W. cases and av. times	343	305		157	300		389	536		111	530		400	652		100	621		399	1043		101	1046	
Av. R. and W.	304						535						646						1044					

	E						F						G					
	Right			Wrong			Right			Wrong			Right			Wrong		
	No.	Time	M.V	No.	Time	M.V	No.	Time	M.V	No.	Time	M.V	No.	Time	M.V	No.	Time	M.V
a.	287	1008	270	52	1153	374	287	592	138	50	522	125	122	809	177	8	843	208
b.	77	1175	272	44	1409	482	40	643	144	51	531	155	141	919	171	30	909	155
c.	14	1161	265	12	1347	453	15	734	166	19	567	153	105	968	183	83	947	209
d.	11	986	395	3	1233	113	6	717	148	32	525	130	3	1203	344	8	1050	200
Total R. and W. cases and av. times	389	1046		111	1268		348	606		152	531		371	899		129	938	
Av. R. and W.	1095						583						909					

Interesting differences in the use of the four categories of confidence occur. The frequency with which each degree of confidence is employed and the percentage of right cases are indicated in the following table:

TABLE V

PERCENTAGE OF TIME EACH DEGREE OF CONFIDENCE IS USED

	A	B	C	D	E	F	G	Bl	Br	H	Av
a.....	13.0	68.4	33.0	90.4	67.8	67.4	26.0	34.9	10.3	9.3	42.0
b.....	34.0	28.2	34.2	8.2	24.2	18.2	34.2	37.8	24.9	32.1	27.6
c.....	20.8	3.4	23.4	1.2	5.2	6.8	37.6	19.6	28.2	33.4	18.0
d.....	32.2		9.4	.2	2.8	7.6	2.2	7.7	36.6	25.2	12.4
Per cent. of right cases ..	68.6	77.8	80.0	79.8	77.8	69.6	74.2	82.0	77.4	83.2	

It is evident that in spite of instructions the various degrees of confidence come in the course of the experiment to have very different values. Some subjects are nearly always perfectly confident, while others are very cautious, *e. g.*, *a* was used 90.4 per cent. of the time by D and 9.3 per cent. by H. Subject B gave no *d* judgments at all and subjects B, D, E and F rarely gave either *c* or *d* judgments. This is an indication of the great difficulty of assigning the degrees of confidence in such measurements, particularly by untrained observers. The averages for the men and women separately show that the women used *a* 47.2 per cent. of the time and the men 34.4 per cent., but the variability is too great to make the difference a significant one.

The correlation between the degree of confidence and accuracy is indicated in Table VI.

TABLE VI

PERCENTAGE OF TIME SUBJECTS WERE RIGHT WITH EACH DEGREE OF CONFIDENCE

	A	B	C	D	E	F	G	Bl	Br	H	Av.
a.....	89	89	93	83	85	85	94	97	95	100	91
b.....	89	56	89	51	64	44	83	84	94	97	75
c.....	73	35	62	33	54	44	56	65	84	85	59
d.....	35		45		79	16	27	48	62	56	41

While the percentages of right cases decreases with the decrease in confidence the degree of confidence is no very accurate measure of the reliability of the judgments. Thus

subject D who was perfectly confident in 90.4 per cent. of the judgments and was right in but 83.1 per cent. of them was nevertheless above the average in accuracy. The relation of confidence to accuracy seems to be an individual matter without any well-defined general tendency. It is impossible, for instance, to say that those subjects who are most confident are either more accurate or less accurate than those who are cautious for the latter may either be very accurate or inaccurate. Fullerton and Cattell[1] found that those observers who had the greatest confidence had the largest probable errors. Griffing,[2] however, found no correlation between confidence and the probable error. No significant sex differences in relation of confidence to accuracy are shown.

A significant correlation does not appear between the time of judgment and its accuracy. Those whose judgments are given quickly are neither more nor less accurate than those whose times are longer, as the following table shows:

TABLE VII

	A	F	G	Br	B	E	D	C	Bl	H
Per cent. of right cases	68.6	69.6	74.2	77.4	77.8	77.8	79.8	80.0	82.0	83.2
Times.....	304	583	909	608	535	1095	1044	646	1021	753

The three most accurate subjects, C, Bl and H, are distributed among those whose times would be short, intermediate and long.

Summarizing the results of the experiments on individual differences the following conclusions are noted:

1. The range of variability in accuracy in discriminating lengths of lines, as determined by these experiments, is as 2:1.

2. The time of judgment varies from one third of a second to one second.

3. The degree of confidence ranges from subjects who are perfectly confident in 90 per cent. of 500 judgments to those who are perfectly confident in less than 10 per cent.

4. While there is a positive correlation on the whole be-

[1] *Op. cit.*, p. 126.

[2] Griffing, H., 'On Sensations from Pressure and Impact,' PSYCH. REV. MON. SUPPLEMENTS, 1895, p. 46.

tween degree of confidence and accuracy the degree of confidence is not a reliable index of accuracy.

5. Subjects whose judgments are given most quickly are neither more accurate nor less accurate than those whose judgments are longer.

6. There are no significant sex differences in accuracy in discriminating lines, nor in time of judgments, nor in confidence, nor in the correlations of time, accuracy and degree of confidence.

EXPERIMENTAL STUDIES OF RHYTHM AND TIME

BY J. E. WALLACE WALLIN, PH.D.
The New Jersey State Village for Epileptics at Skillman

II. The Preferred Length of Interval (Tempo)[1]

A. The Preferred Tempo as Determined by the Method of Paired Comparison of Metronome Clicks

The question as to what particular tempo, or length of interval between auditory impressions, is felt to be the most pleasing, or the most favorable for rhythmizing sounds, has been investigated by various methods, and conclusions have been reached which have not always been in complete accord. It seemed to the writer that the problem ought to be investigated afresh by means of different methods.

In the first investigation recorded here a method of paired comparison of the beats of a metronome (Verdin's make) was employed. The notches on the pendulum of the instrument were so arranged that the following speeds could be obtained by properly sliding the pendulum weight: 40, 48, 56, 63, 72, 80, 88, 92, 96, 100, 104, 108, 112, 116, 120, 124, 128, 132, 136, 140, 144, 148, 152, 156, 160, 168, 176, 184, 192, 200 and 208. A few times, as the rates of the clicks became nearly equal, the weight was moved half way between the notches, so as to halve the difference between the two rates. The half-way point was not determined precisely, but the resulting errors are probably negligible.

[1] An earlier article in this series on 'Qualitative Limens or Grades of Rhythm, and the Difference Limens in the Perception of Time,' appeared in the PSYCHOLOGICAL REVIEW for March, 1911. The remaining articles will detail the results of experiments on the estimation of the mid-points between pairs of different tempos by two methods, and the grouping of metronome clicks into the maximum number of speed categories. Some of the topics discussed here will come up again in these later articles. The metronome experiments described in this article were made in 1905; the observations of the rhythmical responses in the theaters were made from time to time during 1905 and 1906.

The accuracy of the metronome was determined by means of the Hipp chronoscope and a 100-DV fork, particularly by means of the latter. A Deprez marker, electrically connected with the metronome, recorded the contacts made by the swings of the pendulum on the smoked drum of a kymograph along the time line. The metronome was allowed to click off about a dozen beats before the measurements were recorded. It was found that all the rates measured which were faster than 60 per minute were slightly too slow and that the required correction differed somewhat for different tempos. The 1.50 sec. interval (40 beats per minute) was too *fast* by .13 sec., and the 1 sec. interval by .008 sec. The following intervals were too *slow* by the amount indicated: .909 sec. (66 per minute), by .003 sec.; .833 sec. (72 per minute), by .016 sec.; .682 sec. (88 per minute), by .023 sec.; .625 sec., by .013 sec.; .50 sec., by .032 sec.: .416 sec., by .012 sec.; .375 sec., by .011 sec.; .340 sec., by .16 sec.; .320 sec., by .015 sec.; and .288 sec., by .017 sec. (These are all fork measurements, except for .416 and .625.) No measurements were made for some of the speeds selected as the most favorable; in such cases the correction found empirically for the nearest rate was used.[1]

The 'make' and the 'break' checks showed considerable regularity when each was considered separately, but the make checks were closer together than the break, except in two cases, where they were .12 sec. and .024 sec. slower (for the 1.37 and .912 speeds). The differences were as follows: .007 sec. (rate, 60 per minute), .007 (72 per minute), .012 (88 per minute), .004 (120 per minute), .027 (160 per minute), .042 (184 per minute), and .033 sec.[2] (208 per minute). The swings of the pendulum in the two directions were not therefore precisely equal; but the irregularity apparently had little

[1] In 1906 measurements were made by the fork of the rates 80, 96, 104, 112, 124, 126 and 144 (beats per minute). Each interval was found to be too slow by the amount indicated in parenthesis: .75 sec. (by .025 sec.); .625 (.021); .577 (.032); .535 (.026); .484 (.023); .476 (.033); and .416 (.25). By consulting the table it will be noticed that the correction used for .577 was .009 sec. smaller than the empirical result, and .013 too small for .416 sec. These errors are immaterial for the purpose in hand.

[2] In the 1906 measurements the 'break' records were likewise shorter by about the same amounts, except for the speeds (corrected readings) .507 and .775 sec.

influence on the subjects' judgments, for, according to the introspections, the beats were heard as regular, except for Ho for the faster speeds, by H once in the middle of a series, by I once with a slow speed, by K once or twice, and by Wa, J, E, Ea and Z, who at times heard the first few beats as irregular.

Twenty subjects served in the investigation, which was carried out at Princeton, 18 being second-year students in experimental psychology (seniors), one a graduate student in psychology and one a professor of psychology (Professor Warren).

The procedure was as follows: With their backs toward the instrument the subjects listened to pairs of different speeds of clicks, about a dozen clicks for each speed. The initial pair compared always consisted of the extreme rates, 208 and 40. To one half the 208 rate was presented first; to the other half, the reverse. The order was changed during the successive comparisons, so that during half the time the first series consisted of the faster rate and the second series of the slower rate. After having listened to the initial pair, the subjects were asked to state which tempo was preferred, or which was felt to be the more agreeable.

The next step was to compare the selected or preferred tempo with the speed adjacent to the non-preferred tempo. To illustrate: suppose that 208 was preferred to 40; 208 was then compared with 48, then, if still preferred, with 56, and so on until some slower rate was selected. The slower rate was then, in turn, compared with 200; then if preferred, with 192, etc. Each preferred rate was repeated until preference was expressed for the other member of the pair. In this way the speeds, by a series of successive approachments, grew more and more alike until finally the rates were objectively equal.

Here it is of interest to state that the point of equality was sometimes anticipated. Speeds which were objectively different were pronounced equal 16 times by 12 subjects. In half of these mistaken judgments the speeds differed by two beats per minute (the slowest pair confused being 63 and

65 and the fastest 198 and 200); in seven, by 4 beats (the extreme rates confused being 96 and 100 and 204 and 208); and in one case by 12 beats.[1] It will be observed that in all of these instances the differences in the time between the rates are decidedly above the limits of the ability to discriminate different durations of sounds, which according to Helmholtz is 1/132 and according to Exner 1/500 sec.; and that they are also larger than the threshold of difference for empty intervals. Hall and Jastrow found that a variation of 1/60 of empty intervals between clicks 4.27 sec. apart was always correctly noticed, while one of 1/120 was nearly always noticed. Our differences were from 1/30 to 1/5 sec. In the case of these confused judgments there could, in theory if not in practice, be no choice between the rates (except possibly in three cases in which the mistakes were rectified). In all cases, however, the steps were continued until the speeds became objectively equal.

After the speeds had become objectively equal, the subjects listened to about five pairs of the same speeds (without being told that they were equal), and reported their observations respecting the rates and the preference. The introspections showed that the rates were judged equal 37 times, unequal 21 times and doubtful 5 times. The rates were judged to be faster in the first series 8 times as against 14 times in the second series, indicating that the last or most frequent of series of equal sounds appear to be faster. The last series was preferred 16 times, the first 33 times, and no-preference judgments were made 28 times. It is therefore apparent that when the rates of clicks are objectively equal, the tendency is about twice as strong to prefer the first series as the second. That is, the rate which is subjectively slower is more agreeable. There were some subjects, however, who always preferred the last series, some always the first, and some changed their preferences.

These preferences while the rates were objectively equal may be compared with the relative number of preferences

[1] Twice a rate which was two beats slower than the other was called faster, and once when the difference was 8 per minute. The faster rate was judged slower five times, with the rates differing by 2, 4, 12 and 16 per minute (the latter three for I.)

for the two series before the rates became equal. The first was preferred in the gross 309 times, and the second 318. Here the tendency is in the opposite direction, but the difference is so small as to be practically negligible, as was to be expected from the fact that the time order was alternately changed. There were six subjects, however, in which the order of sequence had a considerable influence on the results. Three preferred the first series of beats about twice as frequently as the second (Ho, McD, Wa), and three just the reverse (K, J, E). With nine the number of times the first was preferred more frequently than the second, or vice versa, did not exceed three times each (C, T, L, W, H, Co, Ea, Z, Wy). While it thus appears that the constant time errors can be ruled out for the majority by alternating the series, for a considerable number this appears not to be the case.

After having listened to the same speeds, as just described, the subjects were, finally, asked to give close attention while two longer series of the same speeds were clicked off, and then to state whether they were entirely satisfied with the preferred interval they had selected. The results given in the tables (Tables I. and II.) represent these final preferences. It was found that the great majority (sixteen) were satisfied with the selections they had made, and that only four changed their preferences: three to a slightly slower speed and one to a faster one. This seems to show that the preferences were quite decided. Four stated that it was easy to arrive at a decision, two that it was usually easy, and two that it was sometimes difficult. That there were difficulties involved in making the comparisons, of which the subjects may not always have been clearly conscious, will be seen presently.

To arrive at the final choice, it was found that the subjects were obliged to change their preference on an average of 8.5 times. But the individual differences in this respect were quite considerable. Five changed less than four times (Mo, T, L, E and Z), while seven changed more than ten times (M, C, B, H, Wa, J, E).

At what point in listening to the second series of clicks was the preference made? According to the introspections,

the decision is most frequently reached in the early part of the second series. Nine so reported (one did so usually, and another did so except when the rates were close together), while three made the choice in the middle of the series and five toward the close or after the close of the series. The promptness in deciding on the preferences would seem to indicate that the judgments were largely of the *immediate type*. The subjects were asked whether their judgments were immediate; that is, based directly upon the sensory impressions, or whether they were of the reflective type. Seventeen considered that they were based directly upon the sensations, or upon the immediate effects of the excitations; and five stated that they had to reflect more or less to arrive at a choice (one between the series concerning the first series, and two when the rates approached equality).

If we conclude that the comparison judgments and choices were of the immediate type, are we then justified in inferring that the first series of sounds remained in the mind during the second or part of the second series, as after sensations or primary memories? On this point ten reported that they could easily carry or retain the first speed, two could do so fairly well, and two found it possible to ideate or image the earlier tempos. But a considerable majority—even those who said they retained the first series well—resorted to various expedients by means of which to retain or reinvisage the first tempo. Ten tapped (the first series) with the fingers, some quite frequently, some only when the rates were nearly equal, and nearly all did so with the express purpose of keeping the first series in mind (M, B, J, C, Ea, Z, Ko, Wy, I and T). Movements of the foot (by W, C and Ko) and of the head (by Mo; W, who felt his head swaying, and L, who moved unconsciously) were reported less often, three times each. One used certain muscular strains or tensions for the purpose of retaining the first rate (Wa), and one visualized a line in space for the same purpose (E). While four, on enquiry, stated that they did not vocalize the beats (W, I, H and Wa), three did (L, as 'drum, drum,' and Ko and Wy, as 'tick-tack'), and one believed that he did. This vocalization may

have been merely a tendency toward rhythmical expression, rather than an effort to reinvisage the earlier tempo. Apparently these facts warrant the conclusion that the first tempo was usually not directly (or unaided) compared with the second. The primary memories of the first series were often so vague that some means had to be invoked to revive the sensations. These means were nearly always *motor* in character. The movements or strains used served as substitutes for the first series. Vocalization would have aroused sensations of the same modality as the sensations compared, and would therefore, presumably, have proved a hindrance rather than an aid. The comparisons were then, at times, simultaneous rather than successive. The second tone series was compared with a substitute series of sensations (motor) going on at the same time.

The further consideration of the results may conveniently be made in connection with the following numbered conclusions:

1. We find a very general tendency to subjectively rhythmize periodic auditory impressions of the same intensity, provided the rates are not too slow or too fast. Only two of our subjects failed to rhythmize the beats (McD and H). Two got no rhythm at the beginning of the series (J, T); one got none when he tried to base his judgments on 'pure sensation'; two got none when the rates were very slow or very fast (L, Co); another when they were very slow (T); and one did not get any rhythm for 'some speeds.' One experienced rhythm a few times only, with the slow beats. Three got rhythms with the slow beats more readily than with the fast (Mo, M, Co), and two the reverse (Ho, Z). One asserted that he apprehended all rates as rhythmical. Within the limits of 208 and 40 sounds per minute there is thus no uniform or very clearly marked tendency. Five reported that they perceived rhythms although the beats were apprehended as equally loud, or without accent (Ko, R, Ea, Wa; Co, sometimes; Wa visualized the measures in the form of two long strokes, ——).

The dominant pattern was the trochee (—⏑). This was

obtained by nine subjects (Ho, K, Co, Mo, T, L, J, Z and Wy). The iambic was reported by five (M; Ho, sometimes with the faster rates; B, with all rates; Co, and I. I, however, was not certain). C obtained ⏑ ⏑ — with very slow rates.

In order to ascertain the basis of the subjective rhythms, some of the observers were asked whether they noticed any qualitative difference in the tones from the two swings of the metronome pendulum for any given rate. Three felt that there was a slight difference, without being able to designate what it was (B, H and L); one perceived a difference in loudness (M); one in resonance (Wa); one heard one beat as more muffled (W); four remarked a pitch difference (I; J; H, especially with the slow rates; Co heard the second series with slow rates in a higher pitch, and the first series with fast rates); one "thought of the click on the left side as suspended and on the right side as finished"; and two did not perceive any difference. These qualitative differences were undoubtedly at the basis, at least in part, of the felt subjective rhythm. The pitch element of emphasis, surprisingly, plays the dominant rôle. It has been shown elsewhere that pitch may substitute for the elements of loudness or duration in speech centroids.[1] The metronome used is not now available, and I cannot say whether there was any objective pitch difference in the two clicks. Such differences, if they existed, were probably very small, since many subjects made no mention of them.

The main results will be found in accord with Bolton's findings; he found the same general tendency toward subjective rhythmization and preponderance of the trochee pattern.

2. It is the exception rather than the rule that different interval lengths between objectively equal sounds are felt as indifferent or neutral. One of two contrasted rates or tempos is usually preferred to the other. There were only 17 instances by ten subjects in 700 or more comparisons in which no preference was expressed. This is no doubt

[1] Wallin, 'Researches on the Rhythm of Speech,' Studies from the Yale Psychological Laboratory, 1901, IX., pp. 9–23.

partly due to the fact that the task confronting the subjects was the expression of a choice, although the subjects were not told that no-preference judgments were barred. Granting the force of this criticism, the conclusion will probably hold, that different rates of auditory impressions tend to arouse favorable or unfavorable attitudes in the listener. An analysis of the records enables us to formulate the following rule regarding neutral and doubtful judgments: intervals tend to arouse an indifferent judgment either when they are barely different or when they are very noticeably different; but, while most of the *neutral* reactions are obtained when the rates are nearly alike, most of the *doubtful* judgments are rendered when the rates are very different. Thus there were ten no-preference judgments rendered when the difference between the compared rates was 8 clicks per minute or less (half of these were four) and only 6 when it was greater (namely, 28, 48, 52, 55, 73 and 88 per minute); but there were only two doubtful judgments when the difference was 8 or less as against 16 when it was greater [namely, 16 (by four), 20 (by three), 28, 29, 32, 56, 60 (by two), 61, 64, 72, 77, 80, 81 (by two), 84, 96, 100, 108, 116 (by two), 120, 136 and 168 per minute]. One subject was responsible for nine of the doubtful judgments (Wa), two for six each (Co, E) and two for four each (Ko, Wy). This shows that only a few subjects felt uncertain about a preference, and reinforces the conclusion reached.

3. With objectively similar metronome clicks the preferred tempo for all subjects averaged .519 sec. (see Table I.). or approximately one half second (the median, .579 sec., is higher than the average).

There are, however, considerable individual differences in the preferences. The preferred rates ranged from 1.370 to .305 sec., which makes a difference of 1.065 sec. These are the extreme rates afforded by the instrument, so that it is possible that still slower or faster rates might have been selected. Neither of the extremes seems to possess any advantage. During the initial comparison between the extremes, 40 was preferred 11 times to 208 nine times. But

it is only rarely that either is finally selected; it happened only with 15 per cent. of these subjects. There were only two rates that were preferred twice each, namely .630 sec. (by E and McD) and .305 sec. (by Mo and T).

It is, therefore, difficult to trace any definite *central* tendency in respect to tempo preferences of very wide applicability—because of the varying factors that determine the choice, as we shall see—but we can recognize *types* of subjects. The results can be arranged readily into four groups, according as the preferred tempos exceed 1 sec., fall between 1/2 and 3/4 sec., and fall somewhat short of 1/2 sec. and of 1/3 sec. These four groups may be referred to as *slow*, *medium*, *fast* and *rapid*. The average speed for the slow is 1.169 sec; for the medium, .618 sec.; for the fast, .435 sec.; and for the rapid, .319 sec. Of these values, the medium interval length, which is approximately .60 sec., is evidently the most representative. It is obvious that the faster rates are preferred to the slower, since all but four selected a faster rate than .70 sec., and all except eight a faster rate than .61 sec. To what extent the arrangement of the steps on the metronome pendulum is responsible for these preferences was not determined. The metronome gave 24 speeds faster than .70 sec. (88 per minute) and only 6 slower. But when the records are analyzed it is found that the slower of the pairs of rates was selected in the aggregate by the twenty subjects 351 times, while the faster were selected only 280 times. Thus the 'weighting' of the fast end of the pendulum was overcome, very largely if not entirely, by the more abundant selections of the slower tempos.

If we accept a rate of from .50 to .60 sec. as the most probable value, or the value which will most frequently be obtained, for the preferred tempo, our result will harmonize with a number of earlier findings. Vierordt found a 'neutral' or 'adequate' interval which averaged .62 sec. Wundt's 'indifference' interval averaged .60 sec.; Stevens' interval which could be reproduced the most accurately ranged from .53 sec. to .87 sec. (Johnson, however, experimenting with practice and fatigue, found no indifference point from which

variations did not occur.) In speech it has been found that the average length of the 'complex centroid intervals'[1] is .58 sec.; and that speech series which consist of 'three-syllable sound centroid intervals,'[2] and which average .65 sec., are the most regular or periodic. Speech intervals, because speech is one of our constant psychomotor activities, ought to exercise an important influence upon the selection of the preferred rate. Reference may also be made to measurements of the most favorable period for subjective rhythmization which, according to Martius' result, is 0.50 sec. This coincides with our general average, while Meumann's favorable period for subjective rhythm is somewhat less, .40 sec., and Bolton's considerably more, 1 sec. This wide difference in the findings is probably dependent upon the varying nature of the factors which influence subjective rhythm (see points 4 and 5). Bolton found that subjective rhythmization did not occur when the interval was longer than 1.59 sec. If we assume that an interval will probably not be preferred if it is longer than the limit of subjective rhythmization, it will be noticed that the extreme rate employed in our metronome experiment falls about one fifth second short of the 1.59 sec. limit.

4. The factors which influence the preference for a given rate are numerous and vary more or less with the individual and with the circumstances of the occasion, which accounts for the wide difference found among various individuals. The introspections indicated that one or more of the following factors were operative with one or another observer: physiological irritation or pain, mental disquietude, annoyance, irritation, impatience, or repose; the strain of suspense or expectation, the effort of attention to follow, stimulation to movement, breathing, rhythmical tendency, harmoniousness or discord (tendency to harmonize with ideal beats, tunes, keeping time, preconceived rhythms, suggestions), temperament and associations and suggestions.

[1] The interval from one accented syllable to another, including pauses: cf. Wallin, 'Researches on the Rhythm of Speech,' Studies from the Yale Psychological Laboratory, 1902, IX., 118.

[2] Wallin, as cited, 104 and 110.

The most frequent objection against the slow, or very slow, rates were that they dragged and thus produced a feeling of suspense and a tendency to hurry (B, Mo, C, L, H, W, Wy and I). Three found that they irritated them or made them feel uneasy (K, Mo and Ea), one that they caused a pain in the ear (Mo), and one that they interfered with the breathing (Wa). It is noticeable that only one of these selected a rate faster than .705 sec. While three reported that the slow beats were smoother or firmer (M, J, Ko), and three that they gave a better sense of rhythm (M, Mo, Co), none of these selected a rate slower than .673 sec., while three selected 'fast' and 'rapid' rates.

The most frequent objections to the rapid, or very rapid, rates were that they were irritating or annoying (B, I, M, W), that they were too hurried (J, K, Wy), and that they required too much effort to follow (B, H, L). One found that they produced a headache (W), one that they 'hurt the head' (I), and one that they made the ear throb (Z). Two found them unrhythmical (J, C). They were jerky (Ko), discordant (C), or aroused unpleasant associations (Z, who says he is temperamentally slow), or it was difficult to carry a tune with them (Co). It is noticeable that three of these subjects, notwithstanding the objections, selected relatively fast rates. One found them livelier and more harmonious, and, consistently with these judgments, selected the most rapid tempo (Mo).

The subjects were asked whether they noticed any difference between the faster and the slower series. Three said that the faster seemed louder (K, McD, B), four the opposite (M, Mo, T, J), two that the faster were more distinct (B, Z), three that they were higher in pitch (T, E, Z), one that they made a different noise (I), and three that there was no difference (L, Wy, Ea). None of these factors seems to have exercised any influence upon the selection of the preferred interval.

Aside from the above factors which militated against the selection of rapid or slow rates, the following were the reasons given for selecting the particular rate that was chosen: it

caused the least disturbance or irritation (Wy, Ho), it was the easiest to follow (B, L), it seemed like the average time in music (McD), it gave the most pleasing rhythmical impression (L, who sought a rate that would 'fall into a chord'), it enabled the subject to carry two or three tunes (Co), it was more natural and enabled the subject to keep time to it better (I), it coincided with an 'ideal beat in the head' (Ho), and it fell in with the subject's temperamental disposition (Z).

Listening to the clicks frequently aroused suggestions, associations and images of various kinds which, no doubt, subtly influenced the preference. The faster rates suggested annoying machinery, pile drivers and steam engines, to Z and the slower a swinging pendulum, which was probably more pleasing, as he selected a slow rate. K also imaged a swinging pendulum, but he felt that it was too slow and selected a faster rate. Most frequently imaged was a clock, or the tick-tack of a clock. Of the five who did so (Wa did so only 'indirectly'), all chose slow speeds except one (Z, Ea, Wy, B, K). One imaged telegraph clicks and machinery, (Mo), and another the tick of a clock, which appealed to him, but also the clatter of horses in a race, which appealed more to him (Ko), and both chose rapid rates. One imaged through the test the stone cutters' hammers, chisels and clicks which he had observed before he had entered the laboratory; but for this he felt that he would have chosen a slower rate. He came into the laboratory with a certain set or attitude induced by the experience of the moment, and this determined his choice. One associated his preference with keeping time, singing, the movements of the hands of cheer-leaders and the heart beat, and accordingly chose a fast rate (I). One imaged a man pounding an anvil (Ea), one the ringing of a bell (Co) and one a machine (McD); what influence these suggestions had cannot be said, as the rates of operation for these objects may differ considerably. Sometimes the imagery will change. Thus E first visualized a figure eight motion for the slow ones; then (usually) the motions of a hammer, which sometimes struck higher, and once toward the close a curved hill, with the hammer moving from one part of the

hill to another. W once had an image of a man hammering on an anvil. When the beats were too slow or too fast they were not in unison. He also imaged a hammock; at the beginning he preferred long and slow swings (the records show that he selected the 40 rate three times) and a soothing rate; at the end the hammock was tossed so that it struck at both top and bottom, which made him feel seasick. It was then evidently moving faster; the rate which he selected was .687 sec. These instances indicate that form of imagery and the associations of the moment sometimes exercise a dominant influence upon the preference of tempos. Since the preferred rate depends upon so many and varied factors it seems doubtful whether we shall be able to lay down any *norm* for series of objectively similar sounds, except within wide limits.

5. Apparently there is no definable relation between any particular tempo preference and *musical capacity*, or the ability or inability to sing or play; but the preference seems to depend on the relative predominance of the melodic or harmonic elements or the rhythmic element in one's musical appreciation. Of the subjects who were interrogated, five said that they did not sing, and of these, three are in the medium group (W, E, McD), one in the fast (I), and one in the rapid (Mo). Five sang, two of these being in the medium group (J, M), one in the fast (Ko), and one in the rapid (T). Five sang a little, two being in the slow group (Ea, Wy), two in the medium (E, McD), and one in the fast (Mo). Of those asked, nine said they could carry a tune, three being in the slow group (Z could whistle a tune), two in the medium (J, E) and four in the fast (I, if a crowd was singing). One could not carry a tune (Mo, rapid tempo). Six played an instrument (usually the piano), two being in the slow group (Q, Wy), one in the medium (W), two in the fast (K; C, violin), and one in the rapid (Ho). Five played a little, three being in the medium group (B, Wa, L), one in the fast (Co), and one in the rapid (T). Of the seven who did not play, one was in the slow group (Ea), four in the medium (E, McD, M, K), one in the fast (I), and one in the rapid (Mo). There does not exist, therefore, any clear evidence of correlation between

the favored rate and the ability or inability to carry a tune, to sing, or to play.

A few of the subjects were asked to state which element they enjoyed most in music, the melody, harmony or rhythm. Three preferred the air or melody (Z in piano music), two of these selecting slow rates (Z, Ea), and one a medium rate (J). Two preferred the harmony (Z in orchestra music), both being in the slow group (Z, Wy). One, in the medium group, preferred these two elements to the rhythm (Wa). Three preferred the rhythm to either of the other two elements, and all of these selected fast rates (Co, I, Ko). We may therefore conclude that those persons who chiefly enjoy the melody and harmony in music will favor medium or slow tempos, and those who prefer the rhythm will select fast tempos.

B. The Preferred Tempo as Determined by a Method of Expression (Timing the Spontaneous Responses to Music in Theaters)

In order to study the tempo preferences under the best conditions, we must have recourse to musical rhythms. Certain musical rhythms spontaneously evoke motor tendencies, such as swaying of the body, moving the hands or fingers, chewing (gum), and especially stamping the feet in time with the music. What rates of rhythm or musical tempos tend to produce the most hearty or vigorous motor expressions, the loudest or most diffuse stamping in an audience of unsophisticated people? It may be assumed that the more vigorous the motor responses for a given rhythmical rate, the stronger is the preference for that particular rate.

In order to study the problem by this method to the best advantage, frequent visits were made to the galleries of theaters. The galleries are usually peopled by men and boys who, unaffected by the conventional restraints of the cultured theater patron, allow their feelings to find vent in spontaneous expressions. Accordingly the responses (stamps of the foot) made to different vocal and instrumental selections and stage dances were timed by means of a stop watch, or, in some cases,

an ordinary watch. The timing continued as long as the responses continued at the same intensity for a given selection. The total number of seconds elapsed was divided by the total number of stamps counted, in order to obtain the average interval length between the stamps, *i. e.*, the average tempo. It is possible that in some cases there was more than one response to a measure, so that the rate of the stamps may not always coincide with the duration of the measure, but the records were not made carefully enough to make it possible to work out the distinction with accuracy. The writer also regrets that, having only a slight expert knowledge of musical technique, he is unable to detail the results for the different kinds of musical measures or the different varieties of movements. Nothing has been attempted in this direction beyond grouping separately some of the 2/4 and 3/4 times (Table IV.). It is possible that there may be some inaccuracies in this grouping, as the writer was not always able to recognize the patterns with complete certainty. The problem invites further attention from a psychologist with a technical musical training. There remains to consider the relation of different tempo preferences to different kinds of vocal and instrumental music, to different kinds of dances, to different kinds of measures, and to different kinds of movements of the same measures. In this study the responses were timed during vaudeville, burlesque, minstrel, light opera and glee club performances.

Results.—1. It was possible to grade the responses into four classes, according to the loudness of the stamps or the pervasiveness of the tendency (*i. e.*, the number of persons who fell to beating time). Sometimes the tendency was pervasive although the stamps were not especially loud. These grades are referred to in Table III., as very good (I., a vigorous or pervasive tendency to stamp), good (II.), fair (III). and poor (IV., a feeble tendency to beat time). The average speeds of the responses for these grades are, respectively, .51, .56, .59 and .66 sec., and the general average .58 sec. This gives a difference between the successive groups of .05, .03 and .05 sec., or a difference of .15 sec.

between the best and poorest responses. The differences, in all cases small, are approximately constant from group to group.

2. It is noteworthy that the average for the most vigorous responses evoked by music corresponds precisely with the average for the preferred tempo with metronome clicks (.51 sec., cf. p. 210). It also appears that the averages for the four grades of response to the musical rhythms (from .51 to .66 sec.) correspond quite closely with the most probable value found above for the preferred tempos with metronome clicks, viz., from .50 to .60 sec. (p. 211).

3. Owing to the fact that the large and the small counts may not precisely neutralize one another in the general average, the relative distribution of the averages in ten seconds groups may furnish better indices of the preferred rates than general averages. A comparison of columns (1) to (10) in Table III. shows that the highest number of averages in any of the ten seconds groups is forty; viz., for the rates lying within .41 and .50 sec. This is followed by 32, for the .51 to .60 sec. group, and 26, for the .71 to .80 sec. group. Only ten averages come between .30 and .40 sec.; only twelve between .81 and .90 sec.; and only four between .91 and 1.00 sec. More averages (viz., 50) lie below .50 sec. than above .70 sec. (viz., 42), while the largest number comes between .51 and .70 sec. (viz., 55). It will be noticed that, although there is a large number of averages coming within our most probable value for metronome clicks (.51 to .60 sec.), the greatest number comes within the limits of .41 and .50 sec. This applies particularly to the best grade of responses, grade I., where fifteen averages come within these limits, as against nine between .51 and .60 sec., and none whatever above .80 sec., and where the majority of the averages lie between .41 and .60 sec. (viz., 24, as against a total of 18 for all the other groups). For both the second and third grade of responses, the majority of the averages are contained in the three groups extending from .41 to .70 sec. For grade II. 24 averages come within these limits, as against 18 elsewhere; and for grade III. the proportion is 22 to 8.

These facts, therefore, would seem to warrant the conclusion that the most probable limits within which the preferred tempo will come are .41 to .70 sec. This is larger by about ten seconds on either side than the most probable limits found for the metronome clicks (p. 211). All the above facts, when considered in the light of the additional experimental data cited on page 211, indicate that for most persons under oridnary conditions *auditory rates or musical tempos varying in speed from .40 to .70 sec. will be preferred*, with a pronounced tendency to choose rates toward the faster end of these limits. That the faster rates call out the strongest responses, or preferences, is indicated particularly by the distribution of the averages in the best grade of responses, grade I. We have already pointed out that the tendency with metronome beats is likewise to prefer the faster rates (p. 211).

4. As with metronome clicks, there is a considerable range between the extreme rates which evoked responses. The fastest responses measured averaged .27 sec. (grade III.), and the slowest 1.00 (grades II. and III.). Here the range, however, is smaller than the range for the clicks (from .305 to 1.37 sec.). The fact that fast and slow rates are found in all grades indicates that the preference, or the vigor of the response, is not dependent entirely upon the tempo. The catchiness and familiarity of the music, the distinctness or incisiveness of the accent and the character of the measure, are also important factors. Musical airs which are not catchy will incite to less response than catchy selections, although the tempo is the same. Tempos with barely perceptible accentuation will produce feebler motor reactions than rhythms with a decided accent. Likewise the swing required in waltz music demands a tempo which would be forbidding with other musical patterns. The responses for 3/4 time are uniformly slower than for 2/4 time (Table IV.), the differences for the corresponding grades amounting to .14, .11 and .14 sec. The slowest recorded average for the 2/4 time, .53 sec. for grade III., is faster than the fastest average for the 3/4 time, .62 sec. for grade I. The best rate for 3/4 time, according to these results, is .62 sec.; and for

the 2/4 time, .48 sec. Most of the former patterns were probably waltz times, and most of the latter two-steps.

There are also other factors which account for the distribution of the same tempos in the different grades of response. Sometimes the measurements were made under unfavorable conditions; rainy nights at times made the audience small and unresponsive; and sometimes there were few children present. Tempos which were rated as low on these nights would probably have received a higher rating if the conditions had been propitious.

Table I

Preferred Tempos with Metronome Rates (Final Selection)

Subject	Beats per Minute	Corrected Length of Intervals in Seconds
Ho	179	.348
K	110	.566
M	108	.577
McD	98	.630
C	156	.395
Mo	208	.305
B	88	.705
T	208	.305
L	116	.536
W	90	.687
H	107	.582
Wa	104	.60
J	92	.673
Co	128	.484
E	98	.630
Ea	48	1.18
Z	40	1.37
Ko	144	.428
Wy	63	.958
I	142	.434
Ave		.519

Table II

Preferred Tempos Arranged into Four Groups

I. *Slow*

Subject	Corrected Interval Length Seconds
Z	1.370
Ea	1.18
Wy	.958
Ave	1.169

II. *Medium*

B.	.705
W.	.687
J.	.673
E.	.630
McD.	.630
Wa.	.600
H.	.582
M.	.577
K.	.566
L.	.536
Ave.	.618

III. *Fast*

Co.	.484
I.	.434
K.	.428
C.	.395
Ave.	.435

IV. *Rapid*

H.	.348
Mo.	.305
T.	.305
Ave.	.319

TABLE III

RHYTHMICAL RESPONSES (STAMPS) TO MUSIC IN THEATERS

Grade.	No.	Ave. Sec.	S	L	R	Distribution of Averages.									
						(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
I., V.G.	42	.51	.34	.76	.42	7	15	9	5	6	0	0	22	6	14
II., G.	49	.56	.40	1.00	.60	1	12	11	10	7	5	2	13	14	21
III., F.	32	.59	.27	1.00	.73	1	7	9	6	4	1	2	8	7	15
IV., P.	27	.66	.40	.90	.50	1	6	3	2	9	6	0	7	15	5
Ave.		.58													
Total						10	40	32	23	26	12	4	50	42	55

Grade, grade of response: I., very good (loud stamps); II., good; III., fair; IV., poor (weak stamps). No., number of series measured. The shortest series contained seven stamps; the longest 108; the majority, from 30 to 50 stamps. Ave., average time of interval between the responses, in seconds.

S, shortest average for any series. L, longest average. R, range between the longest and shortest averages.

(1) Number of averages falling between .30 and .40 sec. (2) Ditto, between .41 and .50 sec. (3) Do., between .51 and .60 sec. (4) Do., between .61 and .70 sec. (5) Do., between .71 and .80 sec. (6) Do., between .81 and .90 sec. (7) Do., between .91 and 1.00 sec. (8) Do., below .50 sec. (9) Do., above .70 sec. (10) Do., between .51 and .70 sec.

Table IV

Grade.	Time of Responses in $\frac{2}{4}$ Time.		Time of Responses in $\frac{3}{4}$ Time.		
	No.	Ave. Sec.	No.	Ave. Sec.	D
I., V.G.....	13	.62	15	.48	.14
II., G.	20	.64	9	.53	.11
III., F.	12	.67	4	.53	.14
IV., P.	13	.73	0		

Explanation of signs: see Table III. D, difference (amount by which $\frac{3}{4}$ is shorter than $\frac{2}{4}$ in the corresponding grade).

VOL. XVIII. No. 4. July, 1911.

THE PSYCHOLOGICAL REVIEW.

UNCONSCIOUS IMITATION IN HANDWRITING

BY DANIEL STARCH, PH.D.

The University of Wisconsin

Imitation is one of the neglected topics in experimental psychology. Several investigations have recently been made on imitation among animals, but there is as yet very little work on imitation among human beings.

The experiment upon which I wish to report is an attempt to make precise measurements of the nature and extent of one type of imitation, namely, unconscious or unintentional imitation. By this I mean the unintentional modification in the performance of a specific act due to the presence of a model of that act at the time when it is being executed. For this purpose it is desirable to use an act which shall involve delicate muscular coördinations, and of which a precise record can be obtained. Fine muscular coördinations are more apt to show slight modifications than movements of the larger muscles.

The act of handwriting was selected for investigation because it fulfills both of these conditions. It is a finely coordinated activity and furnishes an accurate record.

The specific problem in view was this: To what extent, if at all, is a person's normal handwriting modified by a model of script seen during the process of writing? Will a person who habitually writes a slanting script, unconsciously make his letters more vertical when he writes from vertical copy? And will a person who ordinarily writes a vertical hand unconsciously make the letters more slanting when he writes from a slanting copy?

The plan of conducting the experiment was as follows. A set of five leaves was prepared of which the first gave the directions of procedure to the student, the second contained a short paragraph of typewritten material, the third was a page of vertical script, the fourth contained slanting script, and the fifth contained unusually large script. These three samples of writing were taken from the school copy books published by the American Book Co. The leaves were fastened together and placed in an examination blue-book.

The experiment was performed simultaneously on a class of 106 students in psychology at the University of Wisconsin. Each student was provided with a set of sample sheets and a blue-book. Before these were distributed, the class was told that each would receive a blue-book which contained several sheets of paper of which the first would tell them what to do, that they should not examine any of the pages in advance but should use them in the order in which they were put together and that they should immediately proceed to carry out the directions. In order to avoid any suggestion of imitation or even of copying, or of anything in regard to the real purpose of the experiment, the directions were printed and the emphasis was rather put upon the desire to obtain samples of handwriting. The directions were as follows: "We desire records of your handwriting. Will you accordingly write out the words and sentences presented on the pages given you. Kindly do this without further questioning or reflection."

After the writing was finished, which took about twenty-five minutes, the class was asked to state whether they had formed any opinion as to the purpose of the experiment and whether they had intentionally tried to make their handwriting like the copy. Among the 106 persons only two purposely tried to imitate the models, and one intentionally made her handwriting different from the models. Their records were not used in the tabulation of the results. The remaining 103 persons stated that they wrote in their usual manner. The majority had no opinion as to the purpose of the experiment and those who did have an opinion did not form it until toward the end of the experiment.

Results.—Each person tested furnished four samples of writing. The first, being written from typewritten copy, was designed to furnish an illustration of his normal handwriting and would serve as a basis with which to compare the other samples. The second was written from the vertical copy, the third from the slanting copy, and the fourth from the large copy.

In order to determine whether these different copies had any effect, and if so how much, two features of the writing were carefully measured, the inclination and the size of the letters.

1. *Inclination.*—Here it was necesary to measure first the slant of the letters in the normal handwriting, and second the slant of the letters when copying the different kinds of script. Three letters were selected for measurement, *l*, *f* and *p*. The measuring was done by means of a specially prepared scale of lines drawn on transparent paper. This scale consisted of a horizontal base line and a series of cross lines making angles with the base line which differed from each other by five-degree steps (see Fig. 1). In measuring, the scale

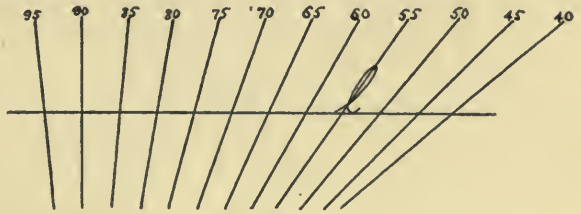


FIG. 1.

was placed over the letter so that the base line coincided with the line upon which the letter was written. That cross line which coincided with the upper and lower points of the letter indicated the angle of inclination. For example, in measuring the letter *l*, the two points which determine the slant are the point where the two lines cross and the extreme tip in the loop.[1]

I will present the measurements of the letter *l* only as the others agree closely with it.

[1] Acknowledgments are due to Mr. A. C. Kroesche for making the measurements.

Average inclination of l in the normal writing .....65.1 degrees.
Average inclination of l written from vertical copy.....68.8 degrees.
Average inclination of l written from slanting copy.....61.5 degrees.
Change from normal to vertical ..... 3.7 degrees.
Change from normal to slant..... 3.6 degrees.
Total range of change..... 7.3 degrees.

The angle of inclination in the slanting model was 40 degrees and in the vertical model 90 degrees.

In three cases out of 103, there was no change. In thirteen persons the change was slightly negative, amounting on the average to 1.3 degrees. This is probably within the limit of chance variation and does not signify a real negative effect. The maximum positive change was 19 degrees in one individual and 24 degrees in another.

Next, the persons tested were divided into three classes: Those whose normal handwriting was vertical, those whose normal handwriting was extremely slanting, and those whose normal handwriting had an ordinary slant lying between the extremes of the first two. The dividing line between these classes had to be drawn arbitrarily. Among the vertical writers were included all those whose habitual handwriting had an inclination of 75 degrees or above. This included twenty-six persons whose results were as follows:

Average inclination of l in the normal writing .....85.2 degrees.
Average inclination of l written from vertical copy.....87.1 degrees.
Average inclination of l written from slanting copy.....79.2 degrees.

Hence, the vertical writers, as one would expect, were influenced more by the slant copy than by the vertical copy. In the former case the change was 6.0 degrees, in the latter case 1.9 degrees.

Among the slant writers were included all whose normal writing had an inclination of 55 degrees or less. This included thirty-four persons whose results were as follows:

Average inclination of l in the normal writing .....50.4 degrees.
Average inclination of l written from vertical copy.....53.8 degrees.
Average inclination of l written from slanting copy.....48.1 degrees.

Hence this class was also influenced most by that model which was most different from their normal writing, namely the vertical. By the vertical copy they were influenced to

the extent of 3.4 degrees and by the slanting model to the extent of 2.3 degrees.

The third class of forty-three persons comprised all whose habitual writing ranged from 55.1 degrees to 74.9 degrees. Their results were:

Average inclination of l in the normal writing ..... 62.6 degrees.
Average inclination of l written from vertical copy ..... 67.4 degrees.
Average inclination of l written from slanting copy ..... 60.0 degrees.

These were influenced more by the vertical than by the slanting model. This is probably due to the fact that the normal inclination of the majority in this class was nearer to the latter model.

The measurements also reveal an interesting difference between the sexes with regard to the force of imitation. The group tested was composed of 28 men and 75 women yielding the following differences:

	Men	Women
Average change from the normal toward the vertical model..	4.0°	3.8°
Average change from the normal toward the slanting model..	2.0°	3.7°
Entire range of change.....	6.0°	7.5°

The women therefore showed a change of 1.5 degrees greater than the men. That is, the influence of the two models upon the women was just 25 per cent. greater than upon the men. The men showed a slightly greater change toward the vertical because more of the men were slant writers.

2. *Size.*—The size of the letters was determined by measuring only one dimension, namely their horizontal width. This was done by measuring in millimeters the length of entire words and dividing by the number of letters. Errors of measurement would thus be smaller than in measuring separate letters. The following results were obtained:

Average width of letters in normal writing..... 4.33 mm.
Average width of letters written from large model..... 4.85 mm.

The width of the letters in the model was six mm. Hence the large model had the effect of increasing the normal width of the letters by .52 mm., or 12 per cent. While this seems a rather small quantity, it would nevertheless amount to half a centimeter in a word composed of ten letters.

One person's writing showed no change in size and four-

teen showed a slight negative change amounting on the average to 0.26 mm. This no doubt also comes within the range of chance variation. Sixty-four persons showed a change in size of from 0 to 1 mm., twenty-two persons from 1 to 2 mm., and four persons from 2 to 2.8 mm.

Summary.—A quantitative method of measuring unconscious imitation has been demonstrated. This method might well be used to study the genetic development of imitation by making the experiment with pupils of different ages.

All persons seem to manifest this type of imitation. Those whose writing showed no change in the inclination did show a change in the size of the letters. The experiment thus emphasizes the subtle way of learning through unconscious imitation.

The measurements show a greater tendency toward imitation among the women than among the men.

There is a positive correlation between change in slant and change in size. That is, those persons who showed a large amount of change in slant also showed a large increase in the size of the letters.

A PRELIMINARY NOTE ON THE CATEGORIES OF ASSOCIATION REACTIONS[1]

BY FREDERIC LYMAN WELLS

Psychiatric Institute, Ward's Island, N. Y.

With the rapid growth of the material centering about the discrete free association test, its interpretations are becoming more definite, and its significant features more clearly understood. Methods of treating the data that shall most clearly and reliably measure these features are the most to be desired.

In the valuable study of Kent and Rosanoff, considerable distrust is expressed of dealing with the responses on the basis of the quasi-logical categories, because of their vagueness and subjectivity. A somewhat similar opinion has been expressed by the present writer. At the same time, it could obviously not be supposed that the further development of the association test would render it possible to dispense altogether with this principle of classification. It is as impracticable to make frequency tables for all association words as it is desirable to state their results in a more significantly descriptive form than they permit.

As the end result of the experience with material dealt with in two recent papers, it seemed that the categories that showed sufficient definiteness to make it possible to preserve them, and sufficient significance to make it worth while to do so, were five in number: (1) the predicates, (2) the supraordinates, (3) the contrasts, (4) a miscellaneous group, roughly one half the total, and (5) the reactions directly dependent upon speech-habits. Compared with the classification previously used, the categories corresponded as shown on the following page.

Any results by the two methods could thus be compared by simply 'telescoping' the former to fit the latter classification. But the analysis of 1,400 further reactions of the Kent-Rosanoff series in company with their frequency co-

[1] The experimental material is from the psychological laboratory of the McLean Hospital.

1. Failure of Response 2. Direct Egocentric	Non-existent in the material thus analyzed.	
3. Egocentric Predicate 4. Judgment of Quality 5. Simple Predicate 6. Subject Relation 7. Object Relation		1. Predicate.
11. Supraordinate		2. Supraordinate.
12. Contrast		3. Contrast.
8. Causality 9. Coördination 10. Subordination 13. Coexistence 14. Identity		4. Miscellaneous, or, essentially, 'Internal-objective.'
also, Doubtful 15. Language-motor 16. Word compounding and completing 17. Pure sound associations 18. Syntactic change		5. Speech-Habit.

efficients disclosed remediable defects in this system, which were now not so much in the nature of a lack of objectivity as in a tendency to obscure what were otherwise indicated to be significant characteristics. In accordance with these, a further modification of the system was indicated, which affected essentially the category above termed the predicates. This subsequent modification has since been tested, in substance, in the original classification of some 6,000 associative responses without suggesting any modifications of importance beyond those mentioned. In view of subsequent contributions based upon them, it may be permissible to print a brief account of the categories as they at present stand.

The categories continue five in number, being now termed the *egocentric*, the supraordinate, the contrast, the miscellaneous, and the speech-habit. The task of defining them may be attempted as follows:

1. The egocentric category ranges in numbers from the largest to the smallest and has included from 2 to 60 per cent. of the responses in single records. In long series of experiments its averages range from 16 to 44 per cent. Upon the definition of this category from the others, hinges the real import of the entire system. It is pretty generally recognized that the essential value of the association test lies in the

indication not of the subject's intellect, but of a perhaps even more important factor of conduct, the extent to which situations are referred to, and interpreted in terms of, special personal experience. In the egocentric category is attempted the separation of these stimulus words so interpreted in terms of personal experience, from those in which the relation is predominantly objective. It has seemed desirable to include:

a. Reactions of modification. These constitute the bulk of the egocentric category. Most of them, further, would be included in the predicate category of Jung and Riklin, and their character may be illustrated in such examples as *cloud-ominous*, *flower-pretty*, *crooked-line*, *red-rose*, *scratch-cat*, *lion-roar*, *money-wish*, *invent-machine*, *weasel-stealth*, *beauty-rose*, *safe-quite*, *almost-grown*, *sing-well*, *never-decide*, *nicely-very* (including the responses *yes* and *no*).

b. Reactions in which the response is a proper name; *e. g.*, *citizen-New York*, *boy-Johnny*, *mountain-Kearsarge*.

c. Reactions in which the stimulus word is interpreted as a proper name; *e. g.*, *eagle-newspaper*, *park-street*.

d. Reactions in which the response involves a pronoun; *e. g.*, *hand-you*, *health-me*.

e. Failures of response or repetitions of the stimulus word.

It is probable that this category should also contain associations where coherence is exceptionally vague, as *whistle-nature*, *dream-behave*, *stem-fishes*, *hammer-court* (Kent and Rosanoff), though they have not been placed here in the present analyses. Their number is insignificant in normal subjects, but the question would be important in dealing with pathological cases, and should be decided in this way.

2. Contrary to usual practice, the supraordinate category is now confined strictly to the individual-genus order. The conception may be defined in such examples as, *priest-man*, *potato-vegetable*, *lily-flower*, *cow-animal*. The category does *not* include part-whole reactions; *arm-limb*, or *arm-member*, are supraordinate reactions within the present conception; *arm-body* is not. This is done to bring it in closer relation to the 'non-specific' category of Kent and Rosanoff with the interpretations thus gained for it. A large number of such reactions seem to indicate an inferior adaptation to the experi-

ment; they tend to disappear with practice. In single records the category may range from 1 to as high as 35 per cent. of all reactions *during initial experiments.*

3. The contrast group in a random series of stimulus words ranges from 1 to 30 per cent. of all reactions. It is composed, of course, of reactions in which the response means the opposite of the stimulus; and is made up of such associations as, *good-bad, trouble-pleasure, scatter-gather, fertile-sterile,* and the like. Its significance is mainly a negative one, in that a markedly egocentric reaction tendency appears to go hand in hand with a prejudice against the contrast reactions.

4. The miscellaneous category (averaging about 45 per cent.) is much the largest of all, save in subjects of markedly egocentric tendencies, in whom this category may exceed its numbers. It is composed essentially of the remaining reactions of the 'inner' type, as Jung, following Aschaffenburg, uses the term. These include, by name, causalities, coördinates, subordinates, coexistences, and identities (synonyms). Reactions of doubtful assignment may also be placed here without essentially affecting results. The predominance of this heterogeneous group indicates a 'concrete' reaction tendency as distinguished from an egocentric one; it has not seemed that anything was gained by separating even its more obvious components.

5. The speech-habits group tends to range up to 15 per cent. of all reactions in different subjects. It is composed essentially of the four sub-classes previously indicated, and is exemplified in such reactions as, *stand-pat, play-ground, tease-sneeze, high-height.* It is the most superficial of all the groups. Doubtless many are suppressed owing to this very superficiality and apparent lack of intellectual dignity. Now and then, of course, such a reaction may occur as a means of escape from emotional suppressions, but this does not seem to be a significantly frequent process.

To sum up, the examination of some 5,000 association reactions has indicated a system of classification preserving itself without suggesting other than trifling modifications, through a material of some 6,000 more. The categories are five in number, consisting of (1) the *egocentric,* including such

reactions as appear to be conditioned by a special subjectivity of attitude, *eine besonders persönliche Stellungsnahme* with reference to the stimulus word; (2) the *supraordinates*, or individual-genus reactions, which possess a certain psychological relation to the egocentric that is, however, too involved to discuss here; (3) the *contrasts*, or associations by opposites, which represent a tendency related rather to the concrete type; (4) a *miscellaneous* group, including the remainder of the 'inner' associations, whose predominance essentially determines the 'concrete' association type; (5) the *speech-habits* category which results, psychologically, from a greater degree of the same 'objective' associational tendencies that determine the reactions of the miscellaneous group.

It is believed that this system of treating the data will, in the simplest way, show the important things about an associational tendency that the quasi-logical method is capable of showing. Its main weakness lies in the insufficient definition of the more highly egocentric responses. As determiners of egocentric reaction type, a few associations such as *working-comfort*, *lamp-smoky*, *woman-Bible*, *health-golf*, *window-joyful*, *memory-painful*, *dream-anger*, *swift-motoring*, *sleep-poorly*, *boy-funny*, *cabbage-stinking*, *wish-disappoint*, *yellow-nice*, *stomach-distress*, may well be worth a copious number of superficial predicates like *sweet-candy*, *baby-small*, *dark-night*, *whistle-blow*, *thief-steal*, *cold-ice*, *sour-lemon*, *smooth-glass*,[1] but there is no way of objectively distinguishing them save on the basis of statistical frequency. The associations of the 'familiar phrase' order must always depend upon the experimenter's estimate of what shall constitute 'familiarity' before the association shall be put in the speech-habits category rather than in a higher one whose conditions it logically satisfies, as the egocentric, *e. g.*, *sour-grapes*. Statistical analysis, where possible, is the only method of dealing with such cases as individual observations; but they do not appear sufficiently important to distort or even obscure the basal trends of association-reaction through fairly extensive series of observations.

[1] From the Kent-Rosanoff material. The first group are all 'individual reactions'; the average frequency of the second is 11.2 per cent.

JUDGMENTS OF PERSUASIVENESS

BY H. L. HOLLINGWORTH
Barnard College, Columbia University

Introductory

The present study represents a two-fold endeavor. In the first place it is an attempt to learn something concerning the variability and consistency of the judgment of interest among different individuals and with the same individual under changed conditions. In the second place it is a preliminary attempt to learn something of practical value concerning the relative strength, permanence and universality of various interests, instincts, and conceptions as a basis of appeal in such practical operations as advertising, salesmanship, etc.

The first problem is of considerable interest for pure psychology. The statistical study of judgments has too long been confined to the comparison of simple sensory stimuli. A psychology which aims to be an account of behavior cannot go far without making a careful study of more complex judgments such as those of appeal and interest. Such a psychology is more interested in performance than in capacity. Psycho-physical measurements have failed to throw much light on the individual's actual performance in competitive life, and this must always be the result of capacity measurements under laboratory conditions. The concrete psychological life depends not on capacity alone, but largely on motive; and a psychology of behavior, as distinguished from a psychology of content (element and attribute), will be interested in performance, in the degree to which motive and capacity sustain each other. Especially will this be true of a psychology which aspires to be concretely serviceable. Such a psychology will find but little use for the introspective method. It will be interested, not in the momentary content of a conscious moment; nor in the descriptive character of

the sensory fragment which may at the moment be the bearer of meaning; nor in the instrument, criterion or vehicle of an act of apprehension, a comparison, a feeling or a choice. It will be most of all interested in the outcome of this moment in the form of behavior, an act, a choice, a judgment, and in the character, reliability, constancy and significance which the outcome of a mental operation bears. Such a psychology must undertake a study of the range, the distribution, the degree of permanence, and the interconnections of such complex judgments as those of preference, conviction, persuasion, satisfaction, amusement, interest, character, resemblance, etc. A merely analytical and introspective psychology is impotent here. What we want to know is the degree to which the individual performance or interest is typical of the average performance or interest of the group; the amount and rate of fluctuation in the individual from day to day and from epoch to epoch, situation to situation; and, in the group, the dependence of these features on the factor of composition. Such an attempt the writer has already reported in a study of judgments of the comic.[1] The present paper, in its first endeavor, attempts to extend the method there developed to the more complex judgments of interest and persuasiveness.

The second purpose of the paper, that of securing information concerning the relative strength of various appeals to interest, attention and response, is justified by purely practical considerations. The Senate Committee has recently attributed the present high cost of living to advertising. Advertising experts estimate that a billion dollars were spent in their industry last year, and that at least three fourths of this amount was wasted,—that is, that the same results in distribution could have been secured at one fourth the cost, if the methods of announcing and soliciting had been more intelligent. Apparently then it is not advertising, so much as bad advertising, perhaps unpsychological advertising, that is to blame for the committee's report. To men who desire to make their copy effective and at the same time economical, the question of the relative strength of various appeals, in-

[1] PSYCHOL. REVIEW, Vol. XVII., No. 2.

terests, instincts and effective conceptions is a live one. The writer has repeatedly been asked by such men to state the relative strength of various appeals in the case of the ordinary man,—to say in how far certain interests are universal, to what degree certain general types are pronounced, how they are distributed, how such interests are conditioned by age, sex, occupation, locality, race, etc. The practical man expects the expert psychologist to possess such information and he has the right to expect much more than we at present are able to tell him. The writer and his students have for some time been trying to answer such questions by concrete experimental methods. The results of most of these are being presented where the practical man can reach them, but certain more technical interests impel the presentation of some of these results in a psychological journal.

The Present Problem

The problem, then, is the measurement of the validity, variability and consistency of judgments directed toward the relative persuasiveness of a series of appeals. In order to eliminate such widely varying factors as habit, association with particular objects, needs, brands, etc., the aim has been to make the appeal abstract. Fifty abstract appeals, each designed to reach a different interest, instinct or line of argument, were written, corresponding for the most part, to the salient points of various widely differing sorts of advertising copy. This series was given the observer along with the following directions:

DIRECTIONS

(Read these directions two times, carefully, before beginning the experiment.)

This is an experiment in the psychology of advertising. Its purpose will be explained after you have finished the series. Each card contains an advertisement of some fictitious article, indicated by a letter-numeral symbol (thus, 3K7). It need make no difference what the article might really be. It may be well to assume that all the cards advertise different brands or makes of the same article,—some ideal, imaginary article, to which any or all of the advertisements might apply.

I. Read all the advertisements through and arrange them in five consecutive piles, in an order of merit,—according to their *persuasiveness*, *i. e.*, according to the degree in which they make you *desire* the article or *convince* you of its merit.

There will thus be five degrees of *persuasiveness*, which might be roughly designated:

1. Most persuasive.
2. Very persuasive.
3. Fairly persuasive.
4. Mildly persuasive.
5. Least persuasive.

Arrange the five piles in a row so that Group 1 is at the top, Group 5 at the bottom, and the three other groups in their respective positions between these extremes.

II. Having done this, turn to the top pile (Group 1) and arrange the advertisements in that group in a strict order of merit,—the strongest in the pile thus getting the first position, the next strongest the second position, and so on.

After the top pile is arranged, treat each of the other groups in the same manner. In this way the whole series of advertisements will have been arranged in an order of merit series with respect to their *persuasiveness*,—with the *most persuasive* at the top and the *least persuasive* at the bottom.

III. Without disturbing your arrangement of the Groups, notify the experimenter that you have completed the series.

The fifty appeals are given in the following list. The cards are here arranged in a final order of merit for a group of forty observers, consisting of twenty men and twenty women. The first pair of figures gives the average value and mean variation for the women, the second pair the same for the men.

1. 1K6,—*Scientific*.—Our 1K6 article is manufactured by approved scientific methods and scientifically tested processes, by technically trained men, working under the constant supervision of experts.

2 –*9.1*, 2 –*8.4*.

2. 1W5,—*Durability*.—Combine utility with durability by using 1W5. It lasts one third longer than the ordinary article. Stands the wear and tear of constant use, combining equal quality with greater permanence and longer service.

1 –*8.2*, 6 –*8.0*.

3. 1F3,—*Sanitary*.—This is the only sanitary 1F3 on the market. Put up in germ-proof, dust-proof, hermetically sealed packages, and made of strictly pure and unadulterated ingredients.

5 –*7.7*, 3 –*10.5*.

4. 2D8,—*Efficiency*.—Actual energy, earning power, is what counts in modern business. The day is past when recognition rested on pull and social influence. 2D8 will increase your efficiency 25 per cent. By no other means can you secure such prompt and sure increase of producing capacity.

7 –*12.8*, 8 –*13.7*.

5. 1T8,—*Time*.—Save the minutes and the hours will save themselves. Time is money. Our latest 1T8 is the biggest time saver on the market. Does in twenty minutes what requires, with other brands, a half an hour.

3 –*8.6*, 14 –*12.2*.

6. 1N6,—*Appetizing*.—Try 1N6. It comes fresh from the field and its appetizing flavor is a treat to the palate. It makes a dainty breakfast, a delightful luncheon and a delicious dessert.

13 –*8.9*, 5 –*8.8*.

7. 2B7,—*Family Affection.*—A final day must come to every man, and no one wants to see his children left dependent on mere accident. You owe a duty of provision and foresight to your family. A 2B7 will guarantee their comfort and security when you are gone.

17—*13.5*, 1—*7.7*.

8. 1Z5,—*Value.*—Absolutely superior quality and finer finish. 1Z5 may cost a little more, but it's worth the difference. One trial will convince.

4—*7.3*, 16—*10.1*.

9. 2L7,—*Evolution.*—Our latest 2L7 is the result of generations of experience and experiment. After years of trial 2L7 stands distinctly in a class by itself as the final product of a long evolution,—the climax of mechanical genius.

11—*10.9*, 12—*9.1*.

10. 2C8,—*Ambition.*—There's always room higher up. Capable leaders are always in demand. Why stay among the incompetent when 2C8 will bring you a better position and increase your salary. The man who uses 2C8 is sure of recognition and rapid promotion.

6—*10.9*, 18—*13.5*.

11. 2F6,—*Self Defense.*—Fore-armed is fore-warned. Your life is always threatened by some lurking danger or another. With 2F6 in your home you are always secure and able to protect the rights and person of yourself and of those whose safety is your chief concern.

15—*0.7*, 10—*11.3*.

12. 1R4,—*Reputation.*—Established in 1870, we have been for forty years the leading manufacturers of 1R4 in the country. We have the longest and most enviable record of any house, in our line, on the continent.

9—*12.8*, 21—*12.0*.

13. 2E9,—*Guaranteed.*—Our well-known trade-mark guarantees quality and satisfaction. All our 2E9 is strictly warranted high grade. Your money refunded if 2E9 does not accomplish all we claim for it.

10—*12.7*, 20—*11.4*.

14. 1P5,—*Stimulating.*—1P5 fortifies the body against inroads of toil and disease gives new life and vigor to tired muscles and nerves, and removes unnecessary strain and fatigue.

12—*9.6*, 41—*10.4*.

15. 1V3,—*Safety.*—Avoid danger by using the only absolutely safety-built, and accident-proof 1V3. Do not court danger by taking chances. This is the only 1V3 in which you get all of the protection and none of the risk.

26—*11.5*, 7—*10.2*.

16. 1E5,—*Popular.*—The name is on all tongues. You will find 1E5 in the ladies dressing room, in the scholar's study, in the nursery, in the kitchens of the humble; in crowded eastern cities and on limitless western plains. Used in millions of homes and everywhere it is on top.

29—*13.2*, 4—*9.9*.

17. 2R5,—*Economize.*—A dollar saved is a dollar earned. 2R5 will save you money. Why not cut down expense items and start a bank account. 2R5 will help you do it.

14—*8.0*, 19—*7.3*.

18. 1Q3,—*Maternal Love.*—Nothing is too good for baby. 1Q3 comforts and soothes the little chap and makes of babyhood one happy play time. Assures the children's health and enjoyment.

18—*13.2*, 4—*9.9*.

19. 1J4,—*Modernity.*—Strictly up to date design, with all the latest improvements. 1J4 is equipped with every advantage and ingenious device known to recent invention.

22—*8.8*, 13—*11.6*.

20. 1C3,—*Health.*—As a general tonic 1C3 is unequalled. It nourishes the system, enriches the blood, builds up firm healthy tissue and gives tone and color to the whole body. Prevents grippe and pneumonia.

8—*10.9*, 30—*15.6*.

21. 1X9,—*Quality.*—Why keep on wasting money when for the price of the ordinary article you can get our superior 1X9. Goes farther and does the work better than any other.

16—*12.5*, 22—*9.5*.

22. 1A7,—*Elegance.*—Nothing contributes so strongly to the luxurious comfort of the modern home as 1A7. Its presence gives dignity and elegance to the whole and creates an atmosphere of daintiness and distinction.

30—*10.9*, 11—*10.3*.

23. 1G2,—*Bargain.*—No 1G2 was ever offered before for the money. As good as any others and only two thirds their cost. We are enabled to offer this proposition only by virtue of our mammoth plant and enormous capacity. Why pay more?

28—*11.0*, 17—*12.0*.

24. 2Q7,—*Sympathy.*—Kindness is the first law of humanity. Much of the pain and discomfort inflicted on dumb animals could be relieved by using 2Q7. Be humane to your beast. Use 2Q7.

37—*15.3*, 9—*9.9*.

25. 2O8,—*Necessary.*—You can not afford to do without 2O8. It is indispensable in your home, in your business, in your recreation. Every man, woman and child needs it constantly.

20—*8.2*, 27—*14.0*.

26. 2W8,—*Middlemen.*—Why pay middlemen's profits? Buy direct from the manufacturer and keep the profits yourself. We make 2W8 and ship straight to the consumer.

23—*11.7*, 26—*10.4*.

27. 2Z7,—*Courtesy.*—Nothing is more discourteous than an offensive breath. 2Z7 cleanses the system, purifies the blood and sweetens the breath.

27—*10.1*, 25—*12.0*.

28. 2T9,—*Remarkable Growth.*—The superior quality of 2T9 is demonstrated by the rapid development of our business.

Total Capital,	1890,—	$15,273.00.
" "	1895,—	85,896.00.
" "	1900,—	240,142.00.
" "	1905,—	703,279.00.
" "	1910,—	3,875,639.00.

19—*16.3*, 36—*11.5*.

29. 1S6,—*Amusement.*—Don't look bored! Buy 1S6. The most side-splitting, mirth-provoking novelty ever devised. Amuses old and young. Affords fun and laughter from morning till night.

34—*10.7*, 23—*10.6*.

30. 2X4,—*Hospitality.*—Don't be content with envying the successful hostess when you can secure the same keen pleasure for yourself. The homes equipped with 2X4 are known far and wide for their generous comfort and open hospitality.

24—*8.0*, 34—*11.5*.

31. 2Y9,—*Youth.*—The fountain of eternal youth has never been discovered, but it has been demonstrated beyond a doubt that 2Y9 restores youthful vigor, quickens the step and gives new life to both mind and body.

24—*8.9*, 34—*13.1*.

32. 2V7,—*Hunting.*—Just the thing for the fishing and hunting trip. Ensures a lively spirit in the field and solid comfort in the camp. No vacation outfit is complete without 2V7.

31—*12.1*, 28—*11.6*.

33. 1O9,—*Social Standing.*—The use of 1O9 is the stamp of the gentleman. It is always found where social standards are high, and is the favorite of men and women of discriminating taste and culture.

38—*9.0*, 24—*12.6*.

34. 2S8,—*Enormous.*—We have the largest establishment engaged in the production of 2S8 in the United States.

Capital, $12,000,000,000.

Factories or branch establishments in every prominent city in the country.

25—*14.6*, 38—*11.4*.

35. 1Y2,—*Cheap.*—Buy 1Y2. Costs just one half the price of its competitors. Why spend two days' wages when one day's work will bring our high class article to your home?

32—*10.1*, 33—*9.6*.

36. 2J9,—*Get the Genuine.*—Avoid substitutes. Many may pattern after us but none can equal us. As a matter of fact 2J9 has many imitators, but there is only one standard, genuine article. Ask for 2J9.

21—*12.0*, 48—*9.4*.

37. 2P6,—*Progress.*—Don't be a dead one. Use 2P6 and be up to date. It is an essential part of every progressive modern establishment.

40—*11.7*, 29—*12.3*.

38. 2A3,—*Sale.*—We are closing out our large stock of 2A3 at a great sacrifice, to make way for next-year's goods. For the next ten days 2A3 will be sold for less than cost. Come early. Don't miss this rare opportunity.

41—*11.7*, 31—*11.7*.

39. 2M5,—*Excel.*—Don't be a wall flower. Use 2M5 and you will be the envy of all your friends. It gives that look of superiority which every one recognizes and respects but which few possess.

36—*11.7*, 42—*13.0*.

40. 2K4,—*Civic Pride.*—We appeal to your civic pride. 2K4 is made in your own city, by local workmen and backed by strictly home capital. Encourage home industry. Use 2K4.

33—*15.0*, 45—*10.3*.

41. 1H9,—*Patriotism.*—Our 1H9 product is made for American consumers, of strictly American grown materials, by an American firm, employing exclusively American labor and American capital.

35—*15.6*, 43—*10.3*.

42. 2G4,—*Union Made.*—We stand for organized labor. 2G4 is a strictly union-made product, built by union labor, of union raised material, and sold exclusively by all union dealers.

49—*14.6*, 32—*11.6*.

43. 1M8,—*Recommendation.*—Here's what the world famous tenor of the Metropolitan Opera House says of 1M9.

"I have used your product constantly and have continued to derive great benefit from it."—(Signed) Enrico Caruso.

46—*14.0*, 37—*14.5*.

44. 1D8,—*Nobby*.—Our 1D8 products are made by our smartest designers, especially for those who love nobby and dressy styles. Exclusive patterns and dashing cuts, unequalled in snap and color.

48—*7.3*, 35—*10.3*.

45. 1B5,—*Style*.—Our new 1B5, is fresh from the center of fashion, representing the latest creation of accepted artists of style, in exclusive designs and dressy patterns, chic and strictly *a la mode*.

47—*5.8*, 40—*7.5*.

46. 1L7,—*Royalty*.—1L7 will be found in most of the houses of European royalty. We are commissioned by official warrant to supply 1L7 to his Excellency, the Emperor of Germany.

50—*8.5*, 39—*12.1*.

47. 2N7,—*Admiration*.—Do you desire the admiration of those you meet? Use 2N7 and you will be the constant center of attraction to adoring and envious eyes. No jewels or marvels of costuming can add so much to your appearance as 2N7.

43—*9.4*, 47—*11.5*.

48. 2H8,—*Imported*.—All 2H8 products are strictly imported and foreign stamped. 2H8 comes straight from European makers and is superior quality is thereby guaranteed.

45—*10.0*, 46—*9.8*.

49. 1U4,—*Beauty*.—Are you as pretty as you might be? No one wants to be homely. The continued use of 1U4 removes the undesirable blemish, beautifies the complexion, renders the form attractive and gives charm to the figure.

42—*8.9*, 50—*9.9*.

50. 2U3,—*Personality*.—Every one desires to be attractive to the opposite sex. 2U3 will give you distinctive presence and engaging personality which is irresistible in its appeal.

44—*10.9*, 49—*10.2*.

The appeals of the foregoing series were typewritten on separate slips of uniform size. Each card bore a single word or pair of words, designed to emphasize the specific character and direction of the appeal, to reinforce the suggestion or argument offered by the text itself, and insure so far as possible, the same attitude in all the observers in the presence of the respective appeals. By employing such copy the following results are secured:

1. Each appeal tends to be single and uncomplicated by other interests.

2. Each is divorced from reactions to any article or brand as such.

3. The elimination of cuts and the use of the same general style and expression lends homogeneity to the group.

4. A wide range of specialized isolated appeals is secured.

As a result of these conditions, the variations and tendencies disclosed by a study of the judgments may be supposed to depend chiefly on the constancy and universality of the interests themselves and on the characteristics of the judgment of such a subjective thing as *interest* or *persuasiveness*. The results will then be chiefly of interest from the point of view of judgment statistics. So far as practical application is concerned, these results can only be said to point the way toward a more definite study of the variation of interest with the variation of the concrete object or commodity in the interest of which the appeal is made. Obviously the arguments which might tend to make a piece of jewelry desirable in the mind of a prospective customer might not be effectively used in the advertising of soap on the one hand or of insurance policies on the other. For each commodity there is probably a more or less definite and distinct set of appeals which is generally efficient. From the practical point of view it is important that each of these general types of commodity be investigated in its own right. Such studies are already in progress, and the results so far obtained have been compared with the actual keyed results of the appeals when they have appeared before the public. It is exceedingly interesting to know that the three such studies already made by the writer of the measurements of persuasiveness, according to the above described introspective laboratory method, have tallied closely with the pulling power of the separate compositions when they are rated according to the keyed results, thus testifying to the validity of the method and to its usefulness for practical purposes. A fuller account of these experiments is to appear in a forthcoming book on *The Principles of Appeal and Response*.

Certain sources of error in such an experiment are at once obvious.

1. It is difficult to keep out of even these abstract appeals some suggestion of special reference. Thus the appeal to *appetite* will inevitably suggest food; some *health* appeals are strikingly medicinal in tone; and doubtless in most cases

there is a more or less pronounced tendency to think of one article rather than another.

2. There is a certain feeling of self consciousness and reserve in submitting honestly to such an experiment, a tendency to place low certain appeals which really bulk large outside of the laboratory, or a tendency towards ideal arrangement strongly suggestive of the inclination to give learned responses in association tests.

Of these two sources of error, it may be said that neither affects in any undesirable way, the character of the experiment as a study of judgment. From the practical point of view only the second error is of importance. And that the danger here is minimal is attested by the fact that the results of such tests are verified by the keyed results. The observers used were thirty women, mostly juniors in Barnard College, taking their second year's work in psychology, and twenty men in Columbia College of corresponding age and class. Twenty of the women made two arrangements, one month apart, without having meanwhile seen the cards. The other ten women and the twenty men made but one arrangement. No time limit was given. Each observer was allowed to work over the material until a satisfactory order had been secured.

Comparison of the two trials of the group of twenty women will thus throw light on the permanence of such judgments and on the consistency of different individuals. Comparison of this group with the group of ten women will show how far the average judgments of such an experiment are typical of the results to be expected from observers of approximately the same class. Comparison of the group of men with the women will reveal such sex differences in these traits as may be present. Separate study of each group will show the degree of variability introduced by the personal equations of the various observers, the general relationships of individual judgments to group averages, etc. The array of figures yielded by such an experiment is a veritable mine of suggestions and material. The chief coefficients of correlation for the present experiment are brought together in the following table.

Table I

Coefficients of Correlation

$$r = 1 - \frac{6\Sigma(d^2)}{n(n^2 - 1)}$$

	Final Average Positions		P.E.
1	20 women with 20 men, one trial	+.624	.06
2	10 women with 20 men, one trial	+.598	.06
3	20 women with 10 women, one trial	+.610	.06
4	20 women, first and second trials	+.903	.02
5	Av. r of girls, first and second trials	+.677	.05
6	Median r of girls, first and second trials	+.719	.05
7	Av. r of 20 girls with average of group	+.432	.08
8	Median r of 20 girls with average of group	+.480	.07
9	Av. of 20 men with average of group	+.588	.06
10	Median of 20 men with average of group	+.610	.06
11	r between agreement with group av. and personal consistency of 20 women	+.285	.12
12	r of final pos. of 50 cards by the method of av. with method of median, 20 women, first trial	+.947	.02
13	r of M.V.'s for the 50 cards	+.743	.04
14	20 women, first and second trials	+.743	.04
15	20 women and 10 women	+.310	.08
16	20 women and 20 men	+.130	.13
17	Av. M.V. of 20 women, first trial	10.8 places.	
18	Av. M.V. of 20 women, second trial	10.6 places.	
19	Av. M.V. of 10 women, first trial	10.5 places.	
20	Av. M.V. of 20 men, first trial	10.9 places.	

Table II

Average M.V.'s of	Best 10	Middle 10	Poorest 10
20 women (1st)	10.10	11.18	10.07
20 women (2d)	9.76	11.93	9.59
10 women	9.37	10.58	8.77
Av. women	9.74	11.23	9.47
20 men	9.84	12.96	10.79
Grand average	9.76	11.44	9.80

Analysis of the Judgments

Coefficients 1, 2 and 3 (Table I.) show the degree to which the final average order of the 50 appeals as judged by one group of observers correlates with the order of the same series according to the average judgments of each of the other two groups. The three coefficients are strikingly equal, being .62, .60 and .61, for 20 women with 20 men, 10 women with 20 men, and 20 women with 10 women. It seems safe to conclude that a coefficient of about + .60 is a fair measure of the likelihood that the results from one group will coincide or confirm the

TABLE III

INDIVIDUAL COEFFICIENTS

Observers	Women		Men
	r with Group Average	r First and Second	r with Group Average
1	+.660	+.808	+.743
2	+.626	.713	.730
3	+.620	.756	.680
4	+.620	.739	.677
5	+.575	.743	.676
6	+.552	.781	.675
7	+.530	.654	.660
8	+.520	.780	.656
9	+.512	.526	.640
10	+.510	.600	.617
11	+.451	.591	.600
12	+.439	.893	.572
13	+.430	.725	.525
14	+.358	.127	.524
15	+.355	.874	.513
16	+.347	.681	.510
17	+.241	.553	.467
18	+.216	.529	.460
19	+.214	.763	.422
20	−.130	.712	.400
Average.	+.432	+.677	+.588
Median.	+.480	+.719	+.610
	r = +.285		

results from another. No sex difference seems to be present, so far as final average rating of the appeals is concerned. The group of 20 women correlate equally well with a group of 20 men and with another group of women while the men correlate about as high with the one group of women as with other.

As compared with this rather low correlation, the two successive trials, a month apart, made by the group of 20 women, yield a high coefficient. Correlating the final average positions of the 50 cards in the two trials gives + .90 (no. 4). This indicates that the average judgment of a given group is rather a stable and significant thing, and is characteristic of that group over a considerable period of time. For single individuals, however, the coefficients are variable, the personal consistency coefficients of the group of 20 ranging for the most part from + .53 to + .89, with one case as low as + .13. This is however the only case that is below + .53. The average

is + .68 and the median + .72. The coefficient for the same individuals' personal consistency thus stands about midway between that of the consistency of a group average and the agreement of the two diverse groups. (See coefficients 4, 5, 6.)

Another set of measurements of interest is found in the figures which show the approximation of the individual's judgments to the average judgment of his group. Coefficients 7, 8, 9, 10 give these figures for the two groups of 20, and Table III. gives the figure for each individual in both groups. The coefficients for the women range between −.13 and + .66 thus giving a total range of + .79, with the average at + .48. For the men the coefficients cover a much narrower range, varying between + .40 and + .74, thus a total range of only 34., a range only 43 per cent. as large as that of the women. The average for men is + .59, the median + .61 being thus about 25 per cent. higher than the same for women. Only 4 women exceed the median for men, while all but 4 men exceed the median for women.

Both of these facts, that of higher correlation and that of narrower range, point in the same direction—viz., toward the greater homogeneity of the group of men. The high coefficients indicate that any one man selected at random will be a better example of the characteristics of his group than will a similarly selected woman of her group. And the narrow range again indicates the tendency of the men, not only to depart but slightly from the type but also to depart in approximately equal degrees from it. Whether these facts point to a greater general variability of women as compared with men, or only to the particular composition of the two groups taking part in this experiment one can not say. But the present method seems to indicate a concrete and interesting way of studying this much disputed question of the relative variability of the sexes, in what may be called the higher mental processes.

A question that has frequently been raised in such studies is that concerning what might be called general judicial capacity. Does the individual whose personal consistency is greatest show any closer approximation to the average of

his group than does the individual whose personal consistency is low? The writer has already presented data on this point[1] indicating that the probability of such general capacity is low. The present study points in the same direction. The correlation between personal consistency and approximation to group average (see Table I.) is only + .29 with a P.E. .12. It seems fair to conclude that such general judicial capacity, when it does show itself, is either accidental or due to the quality of the material used.[2]

The averages of the M.V.'s for the 50 appeals as arranged by the separate groups are strikingly constant, being 10.8, and 10.6, 10.5, and 10.9 places for the 20 women, 10 women and 20 men respectively. That is to say, when the total number of possible positions is 50, the average M.V. is 10, or about one fifth the total range. This would mean, as the writer has previously shown in detail,[3] that only about four degrees of persuasiveness can be given on which the judgment of the group as conditioned by the probable error, would agree. The result here clearly justifies the conclusion reached in the previous study.

In Table I. (coefficients 18, 19, 20) are given the correlations between the magnitudes of the M.V.'s for the separate appeals as judged by the different groups. These figures show how far there is identity in the appeals over which the various individuals of the separate groups differ. Thus for the two trials of the group of 20 women, this coefficient is + .74, showing that the group disagree over the same cards in both trials, and indicating the permanence of the factors on which the M.V. depends, *i. e.*, the specific individual differences. This point is of course farther shown by the high personal consistency coefficients.

But correlating the group of 20 women with the group of 10 women gives a coefficient of only + .31 indicating that the agreements and differences are not common to the two

[1] See 'Judgments of the Comic.' PSYCHOL. REVIEW, March, 1911, pp. 132–156.

[2] See Wells, 'On the Variability of Individual Judgments.'—Essays Psychological and Philosophical in Honor of Wm. James, pp. 511–549.

[3] See 'Judgments of the Comic.'

groups. Comparing the 20 women with the 20 men shows an even lower correlation of only +.13. This means that the women on the whole tend to disagree over one set of appeals and that these are not the same appeals as those over which the men disagree. Just what the difference between the two sets is will be shown later.

A final point in the study of these records or judgments is the question of closeness of agreement at the top of the list, among the preferences, as compared with that at the bottom of the series, among the dislikes. The evidence here, as presented in Table III., is suggestive. Women seem to agree more closely on their dislikes (M.V. 9.4) than on their preferences (M.V. 9.7), but the difference is not large. It is probably reliable and genuine, however, since the relation holds in all three experiments with women. The men, on the other hand, agree more closely on their preferences (M.V. 9.8 as against 10.8 for dislikes) and the difference is considerable. The averages of men and women show no difference whatever, being 9.76 and 9.80. There seems to be a real sex difference here, which, expressed in general terms would be, that men resemble each other more closely in their preferences, while women are more alike with respect to their aversions. This fact throws some light on the previous finding that there is low correlation between the magnitude of the M.V.'s for the particular cards when the variabilities of the women's judgments are compared with those judgments passed by the men.

Applications

Turning now from the study of the judgments themselves and examining the appeals on which the various judgments are passed, a mass of interesting information results. These results, from the point of view of the applied psychologist, merit discussion in detail. In the present paper, however, we must limit ourselves to giving the various tables of measures and briefly summarizing their meaning.

Table IV. gives a statement of the relative persuasiveness of the different appeals for men and women,—their average values and positions. In this table the various cards which

Table IV

Relative Persuasiveness for Men and Women

Appeal	No.	40 Final		20 Women		20 Men	
		Av.	Pos.	Av.	Pos.	Av.	Pos.
Health	3	4.0	1	5.0	2	3.0	1
Cleanliness	1	4.0	2	5.0	3	3.0	2
Scientific	2	6.5	3	6.5	5	7.0	5
Time saved	1	8.5	4	3.0	1	14.0	10
Appetizing	1	9.0	5	13.0	8	5.0	3
Efficiency	3	9.3	6	5.3	4	13.3	9
Safety	4	10.5	7	15.7	11	5.2	4
Durability	2	11.5	8	8.5	6	14.0	11
Quality	2	14.5	9½	10.0	7	19.0	13
Modernity	2	14.5	9½	16.5	12	12.5	8
Family affection	4	15.2	11	21.8	15	8.7	6
Reputation	5	21.1	12	18.4	13	23.8	15
Guarantee	2	21.2	13	14.5	9	28.0	19
Sympathy	1	23.0	14	37.0	23	9.0	7
Medicinal	3	25.0	15	15.0	10	35.0	22
Imitation	3	25.5	16	30.0	19	21.0	14
Elegance	2	26.0	17	34.0	22	19.5	12
Courtesy	1	26.0	18	27.0	17	25.0	16
Economy	5	26.4	19	27.6	18	25.2	17
Affirmation	2	29.0	20	30.0	20	28.0	20
Sport	2	29.0	21	32.5	21	25.5	18
Hospitality	1	29.0	22	24.0	16	34.0	21
Substitutes	1	34.5	23	21.0	14	48.0	28
Clean feeling	4	41.0	24	40.5	24	41.5	25
Nobby	2	42.5	25	47.5	28	37.5	23
Recommendation	2	43.0	26	48.0	29	38.0	24
Social superiority	4	44.0	27	41.2	25	47.0	27
Imported	1	45.5	28	45.0	27	46.0	26
Beauty	3	45.8	29	43.0	26	48.7	29

might be classed under one general heading, such as health, reputation, economy, etc., have been grouped and their average taken as representing the most probable values, of this type of appeal. The second column in the table gives, for each general type, the number of actual cards averaged to give the result found in the table. Since this number varies from 1 to 5, the reliability of the various measures is not uniform.

Columns 3 and 4 give the final averages and positions for the combined groups of 20 men and 20 women. The values range from 4 to 45.8, these numbers indicating the average positions in the list of 50 possible places. The group of appeals, as a whole, falls into three rather sharply defined sections, the series breaking at values 15.2 and at 29.0 at which

points there are wide gaps, which contrast with the gradual transitions of value within the groups. And these sections, moreover, correspond to qualitatively different groups of appeals.

In the first group, with values ranging from 4.0 to 15.2 fall the appeals to health, cleanliness, scientific construction, economy of time, appetite, increase of efficiency, safety, durability, quality, modernity, and family affection. The general characteristic of these appeals is that they are strictly relevant in tone, describe the article precisely or point out some specific value, quality or 'selling point' which it possesses.

In the second group, with values ranging from 21.0 to 29.0, fall the appeals based on the general reputation, guarantee or assertion of the manufacturer, and on a set of semi-irrelevant and more or less social feelings and interests, such as sympathy for others (not family), courtesy, imitation, elegance, hospitality, sport, cheapness, etc. The characteristic of these appeals is that they do not relevantly describe the article but try to connect it with some specific instinct or effective conception. And these appeals are distinctly less personal, more social, than those of the first group.

In the third section, with values ranging (with one exception) from 41.0 to 45.8, fall the rather vague appeals to avoid substitutes, to civic pride and clan-feeling, social superiority, recommendation, the ideals of fashion, foreign origin and finally the beauty appeal. The chief characteristic of this group seems to be that while, as in the second group the statement is semi-irrelevant or incidental, the feeling appealed to is indeterminate and general.

The only considerable sex differences, cases in which the difference in position is say five places or over, appear in the appeals entitled appetite, safety, 'nobby,' family affection, sympathy, elegance and recommendation, which are placed higher by the men; and on time saved, guarantee, medicinal, subsitutes, efficiency, durability, quality and hospitality which are placed higher by the women.

Table V. gives the ten appeals whose M.V. for women is only 9 places or less in both trials, and Table VI. the 7 on

TABLE V

APPEALS WHOSE M.V. FOR WOMEN IS ONLY 9.0 PLACES OR LESS, FOR BOTH TRIALS

Cards	Appeal	M.V.	
		Women	Men
1B	Style	5.8	7.5
1D	Nobby	7.3	10.3
1F	Sanitary	7.7	10.5
1N	Appetizing	8.9	8.8
1O	Social standing	9.0	12.6
1T	Time saved	8.6	12.2
1V	Safety	8.9	9.9
1W	Durability	8.2	8.0
2O	Necessity	8.2	14.0
2R	Economy	8.0	7.3
Averages		8.0	10.1

TABLE VI

APPEALS WHOSE M.V. FOR MEN IS ONLY 9.0 PLACES OR LESS

Card	Appeal	M.V.	
		Women	Men
1B	Style	5.8	7.5
1K	Scientific	9.1	8.4
1N	Appetite	8.9	8.8
1W	Durability	8.2	8.0
2B	Family affection	13.5	7.7
2L	Evolution	10.9	9.0
2R	Economy	8.0	7.3
Averages		9.2	8.1

which the M.V. for men is 9 places or less. These are the appeals on which the numbers of the respective groups agree closely. These appeals are about the same for both sexes. Both tables give the M.V.'s for both men and women for each card. While the final average M.V. for the 50 appeals is the same for women and men, there is considerable difference in the comparative averages of the cards included in these two tables. The men's M.V. (10.1) for cards on which the women agree is greater than the M.V. (8.0) of the women and also greater than the M.V. (8.1) of the men themselves on the cards where their own agreement is close (9.0 or less). The same holds for the women. Their M.V. (9.2) for cards where men agree most closely is greater than the M.V. (8.1) of the men on these cards and also greater than their own M.V.

(8.0) on cards over which they themselves agree closely (9.0 or under).

Whereas the agreements of the two groups (men and women) fall on much the same appeals, the great disagreements fall on different cards. Table VII. shows the cards on which

Table VII

Appeals whose M.V. for Women is over 12 Places in Both Trials

Cards	Appeal	M.V.	
		Women	Men
1E	Popular	13.2	9.9
1H	Patriotism	15.6	10.3
1M	Caruso	14.0	14.5
1V	Reputation	12.8	12.0
2B	Family affection	13.5	7.7
2D	Efficiency	12.8	13.7
2E	Guarantee	12.7	11.4
2G	Union made	14.6	11.6
2K	Civic pride	15.0	10.3
2Q	Sympathy	15.3	9.9
2S	Enormous	14.6	11.4
2T	Growth	16.3	11.5
Averages		14.2	11.1

Table VIII

Appeals whose M.V. for Men is over 12 Places

Cards	Appeal	M.V.	
		Women	Men
1C	Health	10.9	15.6
1L	Royalty	8.5	12.1
1M	Recommendation	14.0	14.5
1O	Social standing	9.0	12.6
1T	Time saved	8.6	12.2
2C	Ambition	10.9	13.5
2D	Efficiency	12.8	13.7
2M	Excel	11.7	13.0
2O	Necessary	8.2	14.0
2P	Progress	11.7	12.3
2F	Youth	8.9	13.1
Average		10.4	13.3

the M.V. for women is 12 places or over in both trials, and Table VIII. the same for men. Both tables give the M.V.'s for each card for both sexes. The disagreement of women fall on appeals to rather general *social* feelings,—patriotism,

popularity, family provision, civic pride, sympathy, union made, recommendation, reputation, etc.—feelings and impulses the absence of which would make social solidarity impossible. On these appeals the men agree (M.V. equals 11.1) much more closely than the women (M.V. 14.2).

The men however disagree over more *personal* appeals, such as efficiency, ambition, economy of time, health, social standing, progressiveness, etc., with an average M.V. of 13.3. But here the women agree more closely, M.V. 10.4. Here again women differ less (10.4) than men (13.3) on men's 'disagreement cards' and much less than on their own 'disagreement cards' (14.2). Men differ (11.1) less than women (14.2) on women's 'disagreement cards' and also less than they do on their own agreements (13.3).

The group of men and the group of women are thus seen to be homogeneous, but the basis of this homogeneity in the two groups is not the same. The men tend to be homogeneous on the basis of social, solidarity appeals, the women on the basis of personal appeals. The greatest individual differences among the men, that is to say, are on the *personal* appeals, the greatest differences among the women on the *social* appeals. Here again is an interesting suggestion, but whether it bears in the direction of selection and sex experience, or toward the temporary motives which operate in such an experiment, cannot be known at this point.

Turn now from the M.V.'s of the separate groups and consider the cases in which the final average judgments of the two groups (men and women) agree closely (within 5 places or less). Table IX. gives the 15 appeals in which this is the case, the + sign indicating that the card was placed higher by the men. These cards fall sharply into two groups, —A, a group of eight which both men and women place high, in the upper third of the range, varying from 1 to 15 with median at 6.5, and B, a group of seven, falling for the most part in the lower third, all but two (25 and 26) being beyond 30 for both men and women, and the median being at 46.0. Which is to say, just as the M.V.'s of a given group tend, in such an order of merit experiment, to be smaller at the

top and bottom of the series than in the middle range, so these two groups agree closely on the strongest and weakest appeals.

Table IX

Cards Showing a Difference of only 5 Places or Less in the Final Orders for Men and Women. Men Higher=+

Card	Av. Pos.	Appeal	
1K	1	Scientific.	
1W	2	Durability.	
1F	3	Sanity	+
2D	4	Efficiency.	
2L	9	Evolution.	
2R	14	Economy.	
2F	11	Safety	+
1Q	15	Child love	+
2W	25	Middlemen.	
2Z	26	Courtesy	+
2V	31	Hunting	+
2N	46	Admiration.	
2H	47	Imported.	
2A	49	Personality.	
2Y	50	Youth.	

Table X

Cards Showing a Difference of 15 or more Places in the Final Orders for Men and Women. Men Higher=+

Card	Av. Pos.	Appeal	
1C	20	Health.	
1P	27	Stimulating.	
2B	6	Family affection	+
2T	28	Growth.	
2J	36	Genuine.	
1V	16	Safety	+
1E	17	Popular	+
1A	21	Elegance	+
2Q	23	Sympathy	+
2G	41	Union made	+
	22 =	Median.	

Do the two groups then differ considerably on their estimates of the cards which tend to fall into the central range as shown by the average judgment of both groups? Table X. shows the ten cards on which this difference is 15 places or more. The median of these cards is at 22 ± 7.5, which means that these cards occupy the middle section of the range.

Just as the repeated judgments of the same individual, and the judgments of the individuals of a group, are more variable in the middle of the scale, so we find that the large differences in the final order for the two groups fall in the middle section.

Column four of Table X. indicates by the + sign the appeal for the "15 or more" cards placed higher by the men than by the women, and by the absence of any mark, the cards placed correspondingly higher by women. We have already seen (Table VIII.) that the men agree most closely on appeals to interests making for social solidarity. The present table shows that the men not only *agree more closely* on these appeals than do the women, but agree also in *placing them higher* in the scale of persuasiveness.

Summary

By way of summary we may say briefly:

1. The judgment of persuasiveness lends itself to concrete measurement and shows certain characteristic constancies and tendencies from individual to individual and from group to group.

2. Such measurements tally with the actual keyed results of the appeals when used as advertising copy.

3. The final orders of strength of appeal as judged by various random groups of observers, correlate with each other with a quite constant coefficient of + .60.

24. The median coefficient indicating the personal consistency of an individual stands about midway between the consistency of a group average and the agreement of two diverse groups.

5. Men correlate with their group average about 25 per cent. more closely than women.

6. The range of variability in the above coefficient is for men only 43 per cent. as large as for women.

7. The correlation between personal consistency and approximation to group average, or what may be called the measure of general judicial capacity, is in such an experiment low, $r = .29$, with P.E. = .12.

8. The average M.V. for all four groups is in this, as in most order of merit experiments in which the judgment is based on general impression, about one fifth the total possible number of positions.

9. A sex difference shows itself in the experiment with respect to the appeals concerning which group judgments agree or disagree. Men agree more closely in their preferences while women are more alike in their dislikes.

10. Generally speaking, the most persuasive appeals are reported to be those presenting strictly relevant descriptions, specific values, qualities or 'selling points.' Next come appeals to specific interests, instincts, feelings and effective conceptions, and finally appeals of a general, incidental or semi-irrelevant kind.

11. Women as a group are more homogeneous when judging the strength of *personal* appeals, but show considerable individual differences in judging appeals made to the instincts or impulses underlying *social* solidarity.

12. Men tend to reverse this relation, being homogeneous where women differ and disagreeing most where women agree.

FROM THE UNIVERSITY OF CALIFORNIA PSYCHOLOGICAL LABORATORY

XIV. The Psychology of Change: on Some Phases of Minimal Time by Sight

BY JOHN M. BREWER, B.S.

The present paper is concerned with the description of an experiment that attempts to bring nearer the answer to the following questions: (1) What is the smallest time-interval between two successive lights when the lights are seen to come not simultaneously but in succession? (2) What is the smallest extent of time that a motion can last, and still be perceived as motion? And the investigation of these two questions, with the hope of bringing out some connection or disconnection of the one group of facts with the other, was confined to a special set of conditions; namely, that in which the two points whose succession or simultaneity is to be observed, are not in absolute juxtaposition but are appreciably apart, and where the space-interval between them is varied as we pass from group to group of experiments. For it seemed an open question whether an advantage that might accrue, let us say, to either motion or succession, when one space-interval was involved, would also be maintained with a different interval of space.

The subject, in a somewhat darkened room, was placed 360 cm. distant from a large screen bearing a narrow vertical slit that could, for the sake of proper fixation, be dimly seen. And in this position he saw either two lights in succession, one directly above the other, or else a moving point of light. These were produced in the following way. Behind the screen and directly in line with the slit was a surface evenly lighted by an electric light; and between screen and lighted surface swung a pendulum, bearing a wide sheet of metal, set into which were devices for producing the appearances

described above. For giving an appearance of succession two small apertures were cut in separate slides that were in reality circular arcs so arranged that the apertures would pass the slit with a varying intervening time, yet with no varying of their distance from the point of suspension of the pendulum; and consequently for any given set of experiments they presented to the observer always the same angular, or vertical, separation. Yet in the different sets of experiments the amount of their vertical separation differed, being respectively 7, 10 and 19.5 mm., or, expressed in angular measure, 6.7′, 9.5′ and 18.6′ of arc.

Instead of the appearance of succession, it was possible to offer an appearance of motion by substituting for the apertures just described a circular card that could be moved at will about its own center. In this circular card there was a narrow slit; and by different settings of the card its slit could be placed at any angle to the slit in the stationary screen immediately before it; and consequently there was presented to the observer the appearance of a point of light moving either up or down. The extent of the motion up and down was carefully limited by opaque surfaces above and below, and made to correspond, in the three parts of the experiment, with the spatial angles above mentioned: 6.7′, 9.5′ and 18.6′. The velocity of the apparent movement, of course, varied step by step with the shifts in the angle of the pendulum slit. The swing of the pendulum was kept constant by the use of magnets, and its speed in the vicinity of the center was carefully calibrated by means of a tuning-fork.

In determining the thresholds for the different orders of stimulation, the method of serial groups[1] was used, 'no-interval' cases being intermixed about equally with the intervals whose thresholds were sought. The threshold was crossed after the manner of the method of minimal changes, and for both orders of sequence (viz., upper light first, and lower light first), and of motion (viz., downward and upward). Each value in Tables I.–III. is then an average of four

[1] See 'Studies from the Univ. of Calif. Lab., IV.,' Psychol. Review, IX., 1902, 444.

'determinations' and into each determination there enter forty to fifty experiments. And since it was often the case that an observer could tell that the lights came at different times, but could not tell which came first, or observed motion, though he could not tell its direction, the two kinds of threshold are here recorded separately—the threshold for the

Table I

Thresholds, Expressed in σ, for Visual Angle 6.7′

Observer.	With Discrete Sequence.		With Motion.	
	Judg. of Succession Merely.	Judg. of Order of Succession.	Judg. of Motion Merely.	Judg. of Direction of Motion.
B	22	29	29	31
L	22	22	21	21
M	54	60	48	48
S	20	22	30	32
W	33	43	22	30

Table II

Thresholds, Expressed in σ, for Visual Angle 9.5′

Observer.	With Discrete Sequence.		With Motion.	
	Judg. of Succession Merely.	Judg. of Order of Succession.	Judg. of Motion Merely.	Judg. of Direction of Motion.
B	22	30	22	22
L	32	32	19	19
M	58	61	33	33
S	35	35	28	32
W	33	33	21	21

Table III

Thresholds, Expressed in σ, for Visual Angle 18.6′

Observer.	With Discrete Sequence.		With Motion.	
	Judg. of Succession Merely.	Judg. of Order of Succession.	Judg. of Motion Merely.	Judg. of Direction of Motion.
B	37	39	30	30
L	28	30	20	20
M	53	60	76	76
S	45	45	24	24
W	43	52	20	20

bare perception of the succession or motion, and that for the definite order of the succession or for the direction of motion. It should be said that the subjective appearance of motion, which Exner noticed, did not appear to any important

extent in making these determinations. Only one subject had this illusion, and he for a very short time only. The results are stated in thousandths of a second.

There is in many cases, as these tables show, a lower threshold when motion is offered than when there is offered discrete sequence. In the thirty pairs of values that are comparable in this respect, twenty-one show a clear advantage for motion. Yet in none of these pairs is there any such advantage as Exner noted. Moreover, it is evident that the smallest visual angle is for the greater number of the observers the most favorable of the three for the perception of the discrete sequence, whether the judgment passed be of sequence merely or be of the order of the sequence. The separation of the lights would here seem to be sufficient to prevent at least any troublesome degree of overlapping of the irradiation circles upon the retina, and at the same time not so great as to render comparison difficult. At the angle 9.5′, such comparison seems already to have become harder and at 18.6′ harder still. For the perception of motion, however, the most favorable space extent for most of the observers was neither the shortest nor yet the longest of the three employed, but the middle distance. Motion through the angle 6.7′ seems to have been too short to be most readily seen as motion; while with the angle 18.6′ there evidently was again some difficulty, though of a different kind and greater—that again prevented the distance from being optimal.

Some idea of the comparative advantages of the different angular extents may be gained from the following table wherein the thresholds for the different orders of perception are stated in the form of averages of the time obtained for all five of the observers.

TABLE IV

AVERAGE THRESHOLDS, EXPRESSED IN σ, FOR THE THREE VISUAL ANGLES

Character of Change Presented.	Visual Angle 6.7′.		Visual Angle 9.5′.		Visual Angle 18.6′.	
	Judg. of Occurrence Merely.	Judg. of Order or Direction of Occurrence.	Judg. of Occurrence Merely.	Judg. of Order or Direction of Occurrence.	Judg. of Occurrence Merely.	Judg. of Order or Direction of Occurrecne.
Discrete sequence	30	35	36	38	41	45
Actual motion	30	32	25	26	34	34

So far as discrete sequence is concerned, there are, however, exceptional observers, like M, who reveal no great difference as we pass from one visual angle to another; or, like B and W, for whom 6.7′ and 9.5′ are practically alike, while 18.6′ shows a distinct disadvantage. And, again, in the region of motion, the general advantage possessed by the medium extent 9.5′ practically disappears with the observer W for whom there is, if anything, a very slight drop in the thresholds as we pass out to the very largest angle, that of 18.6′; and with another observer, S, this same tendency is even more clear, and the longest distance, 18.6′, is now the most favorable of all. Yet the departure of these observers from the precise relations found with their fellows permits one still to say that there is in general in shorter space-extents a condition *favorable* to the perception of discrete sequence, and yet *unfavorable* to the perception of motion. And while the most favorable extent here used for perceiving discrete sequence gives, on the average, a minimal threshold of 30σ, the most favorable extent for perceiving motion gives a threshold of 25σ; the threshold for the perception of discrete sequence is thus about one fifth higher than that for motion. This, however, shows that at least under the peculiar conditions of the present experiment, there is a considerable departure from the relation which Exner believed to exist, namely, that the time-threshold for the perception of succession is about three times as high as that for the perception of motion.

XV. The Psychology of Change: How is the Perception of Movement Related to that of Succession?

BY G. M. STRATTON

The experiments to be described deal with a narrow margin of that wide process by which we are conscious of change. For even with all the interest that has in past years been shown in this complex act, one can hardly say that there is as yet any general agreement as to its inner character or as to its outer connections. It has repeatedly been asserted that change is, at its best, caught by us directly and simply; that when we notice some sudden brightening of a light or damping of a sound or increase of pain or shift of place, the change-experience itself is of a unique quality, like the taste of bitter or the sight of blue, and that at such times the process has all the marks of a sensation. The detection of change, it is held, involves at such times no comparison, no overspanning consciousness that now the object is in one state or condition and now in another, but we become aware of the change in a single and simple throb of mind. And all this has been denied.

In the case of visual movement, to be more specific—for with visual movement alone the present experiments are concerned—orthodox belief has been that there is always present a double act of discrimination, at once of instants and of positions; against which is the belief that the retina is directly responsive to rapid movements of light, and that the movement is perceived without any such complex dealing at once with space and time. The idea of an absolutely direct and simple sensory appreciation of motion suggested by Czermak[1] and to some extent furthered by Vierordt,[2] found a strong

[1] 'Ideen zu einer Lehre von Zeitsinn,' *Sitzungsb. d. kaiserl. Akad. d. wiss. Math.-Naturwiss. Cl.*, Vol. 24 (Vienna, 1857), pp. 231–236.

[2] 'Der Zeitsinn nach Versuchen,' 1868; cf. especially pp. 114 f., where Vierordt says that in perceiving motion both the time-sense and the space-sense are required, but that the processes involving space and time cease to arise clearly in consciousness because they run so smoothly and habit-like, and so the whole takes on a quasi-sensory character. Accordingly, says he, we may as justly use the expression *Geschwindigkeitsempfindungen* as *Zeit- und Raumgrössenempfindungen*.

supporter in Exner, whose argument rests upon two kinds of experimental observation, in the region of space and of time respectively. In regard to space, rapid motion can be discerned, he held, within limits too narrow for space-discrimination; a small disk of light can be seen to move even when its excursion is considerably less than the distance between two such disks of light that, when stationary, already appear as one. *Consequently* (we may paraphrase his argument) *space-discrimination cannot be an essential part of the process of perceiving motion*, at least when rapid. Furthermore, and now passing over into the region of time, motion can be seen as motion even when its duration is far less than the least time that permits us to notice succession. If instead of making the excursion through a shorter and shorter distance, the extent of the excursion remain constant and the movement be made more and more *rapid*, Exner found that, even when the motion occupied a time so brief that two sparks, the one at the beginning, the other at the end of such duration, seemed absolutely simultaneous, the motion was still perceptibly motion. *Consequently* (so runs the thought) *time-discrimination cannot be an essential part of the process of perceiving motion.*[1]

As regards the first part of this argument—the part that deals with space-discrimination—I may perhaps refer to experimental evidence already offered by myself tending strongly (so it seems to me) to show that the facts upon which the argument is based were gathered far too narrowly.[2] If, instead of relying on the two-point method of determining the space-threshold, we employ a method involving the apparent dislocation or coincidence of two parallel lines placed end to end, the power to detect differences of position is approximately the same as that for the perception of movement. And consequently a full half of the ground for believing in the psychic simplicity and independence of movement caves away.

[1] Cf. the argument as set forth in James's *Principles of Psychology*, II., 172. The refs. to Exner are found below.

[2] 'Visible Motion and the Space-threshold,' PSYCHOLOGICAL REVIEW, IX., 433 ff.; and cf. Bourdon: *La perception visuelle de l'espace*, pp. 144 f., with the refs. there given.

I. Previous Experimental Work

Exner's experiments upon the time-factor in the perception of visual movement were in substance as follows.[1] Using close-neighboring electric-sparks, he found that with eyes at a certain distance from the sparks the most rapid succession that could be discerned was one of 44σ. But by more than doubling the distance between eye and sparks and thus proportionately diminishing the visual angle between the sparks and at the same time producing an illusion of movement from one to the other, he found this movement still noticeable when the time-interval was shortened by practice to 15σ. Substituting for the electric sparks flashes of light through two apertures in a screen, Exner could tell correctly the order of sequence when the flashes were 45σ apart; but when the portion of the screen between the two apertures was cut out and otherwise so arranged as to give a moving light between these two localities, motion could still be perceived when its duration was but 14σ. Motion, whether real or apparent, thus had power, it seemed, to reduce the limit below which a visual stretch of brightness appeared as a synchronous and uniform illumination. Exner's data for this conclusion were obtained from foveal vision; with eccentric vision he found it impracticable to exclude the appearance of motion; he was thus unable to compare in this region the times of just-visible motion and of succession apart from movement.

Charpentier, who next in any significant way approached the problem, employed a method much the same as the second of Exner's described above, yet with this important difference, —that instead of producing discrete succession by separated, although neighboring spots of light, he arranged a vertical slit, of which he could illuminate independently the upper and the lower half, and thus the sensations whose sequence or simultaneity was to be caught appeared in absolute juxtaposition. He found: (1) that if the time-interval between the halves be slight, they seem illuminated simultaneously;

[1] Exner, 'Experimentelle Untersuchung der einfachster psychischen Processe Pflüger's *Archiv*, VII., 601 ff.; VIII., 526 ff.; XI., 403 ff.; 'Ueber das Sehen von Bewegungen und die Theorie des zusammengesetzten Auges,' *Sitzungsb. d. kaiserl. Akad. d. Wissens. Math.-Naturw. Cl.*, LXXII., Pt. III (Vienna, 1875), pp. 156 ff.

(2) with increase of this interval the illuminations seem distinct in time, but it is impossible to say which comes first; (3) only with still greater increase of the time-interval can one say which is first; and (4) the absolute duration of the illumination of each half seems not to affect this detection of sequence. On the average (and apparently from but a few experiments) he found a noticeable difference when the beginnings of the lights differed by 27σ.[1] With regard to Charpentier's discussion of the error we make in the appreciation of succession—the illusion that successive lights are simultaneous—it was contended by Bloch that this error entirely disappears if the successive stimulations do not fall upon identical parts of the retina,[2]—a surprising criticism, apparently based neither on any right understanding of Charpentier nor upon any reliable experiment of Bloch's own. Charpentier, in reply,[3] after clearing up the matter of method, says briefly and with restraint, that while he cannot deny that the succession may continue to appear to the very end, he is quite unable to notice it himself. Neither Charpentier nor Bloch seem to have made more than a few trials nor to have concerned themselves with obtaining comparable data in the region of movement, and so of clearing up the problem more directly before us.

This problem was, however, held distinctly in mind by Stern,[4] who arrives at a conclusion opposed to that of Exner. But since his own experimental contribution is confined to the spatial rather than to the temporal side of the question,[5] it hardly need here be reproduced.

[1] Charpentier, 'Sur l'appréciation du temps par la rétine,' *Séance et Memoires de la Societé de Biologie*, Vol. IV., 8th Series (Paris, 1887), pp. 360–1; 'Nouvelle note sur l'appréciation du temps par la rétine,' *ibid.*, pp. 373–5.

[2] Bloch, 'Note sur les sensations visuelles,' *ibid.*, pp. 391–3.

[3] Charpentier, 'Note sur le synchronisme apparent,' etc., *ibid.*, pp. 447–8.

[4] 'Die Wahrnehmung von Bewegungen vermittelst des Auges,' *Zeitschrift für Psychologie*, etc., VII. (1894), 321 ff. Stern is in the neighborhood of the time-side of the problem only when he considers the most favorable *rhythm* of motion if the motion is to be noticed. Cf. my own discussion and opposing results, in 'Visible Motion and the Space Threshold,' PSYCHOLOGICAL REVIEW, IX., 433 ff.

[5] This is true also of the work of Basler, 'Ueber das Sehen von Bewegungen,' Pflüger's *Archiv*, 1906, XV., 582, 1908; CXXIV., 313.

Weyer, experimenting with successive electric sparks, given (it would seem) from a single sparker and consequently appearing at one identical point, brought out some interesting facts concerning the detection of sequence; but his interest was not directly in our problem, and he made no attempt to obtain in the field of movement data with which his results upon succession might be compared. He found, however, that in starting with so brief an interval that the two sparks appeared as one, there were certain definite intermediate stages before this appearance of precise singleness of spark yielded to a clear appearance of succession. The apparently unitary illumination began to drag, or 'persist' when the interval was somewhat increased—to about 12σ, with daylight adaptation. With still greater increase—to an interval of $.25\sigma$–30σ—there came a slight flicker. Finally, when the interval was increased to 43σ–53σ, the two sparks appeared in clear succession.[1]

With Bourdon there was distinct interest in our particular problem, and he accordingly gathered data both of succession and of movement. He used small areas of white paper attached to a revolving drum, so arranged that they produced an appearance now of motion and now of mere sequence of neighboring lights. When the conditions were on the whole most favorable, he found motion recognizable by direct vision at a duration of 27σ. With discrete succession instead of motion, he found it impossible to pass judgment when the interval was only 16σ; his right judgments exceeded the wrong when the interval was 20σ; when the interval was increased to 48σ, he recognized with certainty the *order* in which the lights disappeared. His conclusion is that the perception of motion has no such preëminence over that of succession as Exner claimed; indeed, the thresholds of these two kinds of perception come to nearly the same measure.[2]

[1] Weyer, 'Zeitschwellen gleichartiger und disparater Sinneseindrücke,' Wundt's *Philosophische Studien*, XIV., 616; XV., 67.

[2] Bourdon, *La perception visuelle de l'espace*, pp. 187 ff. Marbe (*Theorie der kinematographischen Projectionen*, 1910, p. 62) has added stlll later experiments, but again upon but one of the two sides important for the present question. He finds that when a series of 13 miniature incandescent lights flash in succession at a rate wherein the interval between the flashes of adjacent lights is reduced to 12σ the impression of movement ceases.

Only Exner and Bourdon, then, maintained in the two regions that must of necessity here be compared, a single unchanging method applied under inner and outer conditions that also in the main remained unchanged. And their excellent work, I have ventured to think, still leaves room, even apart from a certain conflict of outcome, for some doubt and inquiry. With Bourdon, for instance, it is clear that exactly the same conditions were not maintained both in his experiments upon succession and in those upon motion. The lines that passed the slit were not alike in the two cases; and this might have affected the apparent intensity of the sensations; the slit through which the stimulus appeared was, in the case of the motion, horizontal and 1 mm. wide, in the case of succession it was vertical and 0.4 mm. wide; the distance between the observer and the lights was different in the two cases. All these discrepancies should be removed, and the two series to be compared should run on abreast to avoid any possible advantage from the difference of training and practice. In particular it has seemed to me desirable to regard as an open question what the effect of certain variations of method would be; and farther, just what space relation should subsist between the successive lights, if the conditions were to be most favorable for the detection of succession. Will succession best be observed when the successive spots of light are closer together or farther apart; when they are in juxtaposition or when some distance intervenes? We should discover the most favorable conditions for each class of observations before we could finally decide as to their relative superiority.

It would be idle to claim that the programme finally adopted covers the ground; all that should be said is that it aims to clear up an obscure point or two. A part of it has been carried out by an able student of mine, Mr. Brewer,—the part dealing with successions that are not only temporally but spatially separate. And his report accompanies and may be regarded as an intimate part of this discussion.[1] His experiments had shown a regular reduction of the time-

[1] Brewer, *The Psychology of Change: On some Phases of Minimal Time by Sight*, in the present number of the PSYCHOLOGICAL REVIEW.

threshold for succession, as the visual angle was reduced. For myself, I felt tempted to push this reduction to its very limit by making trial, in the region of time, of a slight modification of method that has proved of marked effect in studying movement upon its spatial side. For it will perhaps be remembered by some that a change in the manner of testing the space-threshold, whereby lines were placed end to end, instead of side by side, had reduced the space-threshold at least five-fold and perhaps ten-fold; had reduced it from a visual angle of 30″ with stars, or of 60″ if we keep to lines, to a visual angle of approximately 7″.[1] Here in placing the lines end to end, there is perfect proximity of the impressions to be compared, yet without the easy fusion which befalls lines side by side. An analogous procedure in the realm of time would be to have the succession of flashes come, not upon the same points of the retina where persistence of impression would conceal the time-interval, but upon adjacent points, yet neither with confusion by overlapping circles of irradiation nor with any insistent illusion of movement. This seemed attainable by means of a narrow slit, half of which should be lighted at one instant, and half at another,—the time-interval between these illuminations to be variable and definitely known. The plan, then, involved the method already employed briefly by Charpentier, and at the same time the gathering of exactly comparable data regarding movement.

II. Experiments with Pendulum Device

In the present experiments two different arrangements of apparatus were employed. The first group of experiments was with the pendulum described in Brewer's paper already mentioned;[2] but with attachments other than those he used. Imagine a plate of aluminum just above the pendulum's bob, lying in the plane of oscillation. In this plate and at about a distance of 1 m. from the point of suspension of the pendulum there was a vertical radial slit 5 cm. long, with a mean

[1] 'A New Determination of the Minimum Visibile,' Psychological Review, VII., 249; 'Visible Motion and the Space-Threshold,' Psychological Review, IX., 433 ff. Cf. Bourdon, *La perception visuelle de l'espace*, pp. 144 ff.

[2] See pp. 25 f.

width of 4.5 mm. later narrowed to 1 mm. The pendulum with this radial slit swung behind a stationary screen which also had a vertical slit with parallel sides 2 mm. apart and long enough to expose the full length of the slit behind. Back of both screen and pendulum was ground glass evenly illuminated by six 16-c.p. incandescent lamps. With certain adjustments the observer, at a distance of 2 m., perceived a momentary illumination of the vertical slit in the stationary screen; while with other adjustments the upper and lower half of the radial slit in the pendulum could be mutually so displaced as to present to the observer the two halves of the slit illuminated in quick succession. The exact point in the front slit where these two halves had their common border was, for the sake of preparatory fixation, indicated by white horizontal arrows, while the time-interval between the illumination of the two halves could be exactly controlled by a scale obtained chronographically.

The impression of movement, for the companion experiments, was obtained by another attachment essentially the same as that described in Brewer's paper.[1] It need hardly be said that here the same width of slits and intensity of illumination of background, was maintained as in the experiments on succession, and the extent of the motion was identical with the length of the two illuminated halves then used.[2] And in all cases the two sets of experiments were so arranged as to exclude any difference of practice or fatigue in the two groups. There were two observers in the present group of experiments, as well as in a second group later to be described. To Dr. Brown, who gave unstintingly of time and good will in these none-too-interesting tests, I acknowledge my great indebtedness.

Since the tendency here is to see every impression as motion and indeed as motion in some particular direction,

[1] See p. 258.

[2] Brewer's experiments, it will be seen, showed that the optimal excursion for perceiving *motion* is, unlike that of succession, by no means near zero. With some it seems to lie at least as far out as a visual angle of 18.6′. The motion in the present experiments was some five times this extent, and in all probability included the optimal excursion for my observers.

the 'method of serial groups'[1] was used instead of the more common gradation method. The procedure was '*unwissentlich*'; and but a single movement, or pair of lights in succession, was offered for judgment. At first an attempt was made to distinguish, in each judgment, not only between motion and succession but also between the two orders of succession (upper first; lower first) and the two directions of movement (down; up). But prepossession soon proved too strong, and so wide a range of intervals was needed to catch all phases of these judgments that I finally abandoned the attempt to obtain thresholds for the correct perception of the order of

TABLE I

OBSERVER B

Thresholds expressed in mm. (*1 mm.* = *1.5σ*)

Order of Presentation	With Two Successive Lights		With Moving Light	
	Above-below	Below-above	Downward	Upward
With wider slit.	12 11 18 15	17 17 15 15	35+ 35 45	25 30 25
Average.	14	16	38.3	26.7
With narrower slit.	15 20	20 22	40 35	25 20
Average.	17.5	21	37.5	22.5
Combined average.	15.2	17.7	38	25
Average for the two orders of presentation.	16.4		31.5	

succession and for the direction of motion, which are unquestionably distinct from the thresholds for the perception of mere succession and mere motion. I felt content to note the point at which actual succession on the one hand and motion on the other, no longer seemed sheer simultaneity of illumination. Yet the observers were, through a large part of the present group of experiments, more or less concerned to make out more exactly the nature of whatever departure from simultaneity they noticed, and so their judgments were not quite untrammelled and homogeneous. Yet they should perhaps be entered, since in several ways they are worth

[1] See PSYCHOLOGICAL REVIEW, IX., 444 ff.

comparing with the group to be spoken of some pages hence. Each 'determination' given in the tables accompanying (Tables I. and II.) represents never less than 50 nor more than 100 separate judgments. In all, there were between 1,500 and 1,600 judgments in this group, apart from many preliminary experiments and the half dozen judgments at the opening of each hour for 'warming-up.'

TABLE II

OBSERVER S

Thresholds expressed in mm. (1 mm. = 1.5σ)

Order of Presentation	With Two Successive Lights		With Moving Light	
	Above-below	Below-above	Downward	Upward
With wider slit.	9 8 10 10	20 20 20 20	10 15 10	20 20 15
Average.	9.2	20	11.7	18.3
With narrower slit.	12 13	13 23	8 8	15 17
Average.	12.5	18	8	16
Combined average.	10.3	19.3	10.2	17.4
Average for the two orders of presentation.	14.8		13.8	

The tables show that for Observer B. there was a decided advantage on the side of succession, and for Observer S., no marked advantage either way, although a slight advantage appears on the side of motion. Whatever gain here accrues to 'succession' is not in any wise due to an illusory motion when mere succession was offered. On the contrary, with both observers the presentations, whether of succession or of motion, when just rising above the threshold were perceived as *succession* far more often than as motion. The usual course of the judgments was this; with the longer times there was a clear discrimination between succession and motion; and then, with briefer times, the presentation was called 'successive' or 'different,' meaning that it was not a single simultaneous illumination. Even with actual motion, therefore, the observer failed to notice the motion, but noticed

only non-simultaneity. And this lower threshold is entered in the tables even in the case of motion. Where, with B. for example, the thresholds for motion are entered as 30σ and 35σ, this indicates the point at which a departure from simultaneity was evident; motion was discerned as motion only at 40σ and 45σ respectively; and with S. the detection of motion required a value even higher in its ratio to that of mere succession.

Furthermore, there is a difference, in some cases quite marked, between the thresholds for the different orders of presentation. Thus for both B. and S. the threshold is lower when, in the case of succession, the upper half of the slit is illuminated before rather than after the lower. And with motion also the direction affects the threshold, although here the two observers do not show a like divergence, since B. best catches motion upward, while with S. motion downward has the advantage. But of this phenomenon more will be said in connection with the later experiments.

III. Experiments with Wheel Apparatus

The set of experiments just reported seemed to me inconclusive because of a growing doubt both of the apparatus and of the method. The puzzling disagreement of results obtained in the different orders of presentation, though the observer did not have to decide which order was given him (to which attention has just been drawn), made me perhaps unwarrantably inclined to believe that this inconsistency might be due to some want of rigidity in the instrument. And hesitation arose, too, from the very fact that the values obtained bore, at least in the case of one of the two observers, a relation so decidedly the reverse of that in the classic results.

Consequently an entirely new apparatus was constructed by a skilful instrument-maker—a pendulum-wheel suggested by a device of Sanford's,[1] but departing from it in several respects. From a sheet of aluminum ¼ in. thick, there was cut a narrow circular rim 20 in. in diameter, having thin poke-like connections with its center. At the center there was a steel axle with ball-bearings resting upon a rigid frame and base,

[1] Sanford, *American Journal of Psychology*, VI., 581 ff.

upon which there were levelling screws and a circular spirit level. To the rim of this skeleton wheel was carefully fitted a weight and catches, so that with the weight high upon one side, when one of the catches was released, the wheel revolved until the weight rose high upon the opposite side; and there at its extreme excursion the wheel was caught and the return of the weight prevented. In preparation for the next fall, the rotation of the wheel was completed by hand, and by means of the catch the weight was held high upon the side whence it first fell.

Upon the axle of the wheel was a screw holding two large black bristol disks, one of which was fixed by additional screws upon the rim, the other being movable and to be set instantly at any position desired. In these disks there were in principle, the same devices for producing succession and movement as with the pendulum before. Thus any order and interval of succession, any direction and speed of movement, could be had at will. Proper scaling in thousandths of a second was provided beforehand by tuning-fork records on the disk, the tuning-fork itself having of course been first carefully corrected.

The method was such as to avoid any extraneous advantage accruing, by practice or fatigue, to either of the forms of perception. Experiments upon succession and upon motion were from the first mingled in equal number at every sitting; nor did the observer himself know in any case whether the stimulus to be offered him involved motion or succession merely, nor was he told this after his judgment had been given. But a single movement, a single pair of successive lights, were given for any one judgment. And further it seemed advisable, in view of what had happened in the preceding set of experiments, to simplify the task of the observer and render his judgments more direct and trustworthy. Hitherto, as already has been indicated, the judgments showed some embarrassment before the several possible queries: (1) Was the impression throughout its stretch simultaneous, or was there some departure from perfect simul-

taneity? (2) If there was such a departure, had it the character of a successive illumination of the two halves of the slit; or of a motion? (3) If a succession, did the lower half of the slit flash before or after the upper? (4) If a motion, was this upward, or down? Now the observer is ill at ease before so many questions, and his nicety of judgment is apt to be affected by the attempt to keep his mind at once open to so many aspects of the impression, and in the effort to answer secondary questions the answers to primary are often obscured or lost.[1]

Accordingly in this whole group of experiments, even from the start, the observer was asked to confine his attention to the first of the four questions just listed. Additional judgments were occasionally volunteered; and although noted, they were not encouraged. The method again was, at bottom, that of serial groups, inasmuch as a number of fixed intervals or durations were selected and repeated; and interspersed with these were frequent cases where the interval or duration was reduced to zero. Yet instead of adhering to a particular interval or duration until its group was complete, and exchanged for a new group and so on until the threshold had been passed, there was no attempt to determine a threshold in the accepted sense, but only by a full display of right and wrong cases, with each interval or duration, to reveal the relative facility of perceiving movement and succession. The following partial series of actual stimuli presented, where the distribution is quite by chance, will perhaps make clear the procedure just described.

0σ motion (=simult. illum. of entire slit).
10σ succession, upper-lower.
20σ motion, upward.
30σ succession, lower-upper.
0σ succession (=simult. illum. of entire slit).
10σ motion, upward.
0σ succession.
25σ succession, upper-lower.

[1] See the argument offered by Brown ('The Judgment of Difference,' *University of California Publications in Psychology*, I., 28 ff.) for a simple alternative in the question —an argument that has had great weight with me.

TABLE III

(JUDGMENTS UPON SINGLE PRESENTATIONS; DIRECT VISION)
OBSERVER B

Discrete Succession

Interval[1]	Successes	Failures
30σ	12	0
25σ	12	0
20σ	13	1
15σ	10	4
10σ	10	4
5σ	6	8
Totals.	63	17

Motion

Duration	Successes	Failures
30σ	8	4
25σ	9	3
20σ	10	4
15σ	6	8
10σ	2	12
5σ	6	8
Totals.	41	39

Of 72 instantaneous illums., 50 were judged 'simult.,' 22 'non-simult.'

TABLE IV

(JUDGMENTS UPON SINGLE PRESENTATIONS; DIRECT VISION)
OBSERVER S

Discrete Succession

Interval	Successes	Failures
30σ	10	2
25σ	11	1
20σ	12	0
15σ	9	3
10σ	5	7
5σ	6	6
Totals.	53	19

Motion

Duration	Successes	Failures
30σ	12	0
25σ	9	3
20σ	10	2
15σ	11	1
10σ	7	5
5σ	6	6
Totals.	55	17

Of 72 instantaneous illums., 47 were judged 'simult.,' 26 'non-simult.'

[1] 'Interval' here and elsewhere means the time between beginning and beginning, or end and end, of the two successive flashes.

20σ motion upward.
15σ succession, lower-upper.
5σ motion, downward.
0σ succession.
30σ motion, upward.
0σ motion, etc.

The following tables exhibit the results of these experiments.

Method of Paired Presentations.—Before discussing these results, let me offer also those of another method, used as a check. Here the apparatus and procedure was in all respects as just described, except that there was given, in sequence, two presentations; namely, a zero case—where there was absolute simultaneity of illumination—and a case where there was actual succession or motion. The observer was told that there would be two presentations, one of which would be a simultaneous illumination of the entire slit, the other a departure from such simultaneity; and that his task was simply to tell whether the simultaneous illumination occurred first or second in the pair. The two presentations were then given, with as little time between as could be with the adjustment required (about 4 seconds), and no judgment was passed until after the appearance of the second of the pair. The

TABLE V

(JUDGMENT BY PAIRS; DIRECT VISION)

OBSERVER B

Discrete Succession

Interval	Successes	Failures
20σ	11	1
15σ	10	2
10σ	8	4
5σ	6	6
Totals.	35	13

Motion

Duration	Successes	Failures
20σ	7	5
15σ	8	4
10σ	8	4
5σ	4	8
Totals.	27	21

Table VI

(Judgment by Pairs; Direct Vision)

Observer S

Discrete Succession

Interval	Successes	Failures
20σ	14	0
15σ	10	4
10σ	11	3
5σ	11	3
Totals.	46	10

Motion

Duration	Successes	Failures
20σ	13	1
15σ	13	1
10σ	7	7
5σ	6	8
Totals.	39	17

observer was ignorant of the actual order, and this order itself was irregularly changed. The following tables exhibit the results obtained.

Examination of the results of these two different procedures discloses that with neither of the observers, nor by either method, is there an advantage for motion over succession. And consequently the new apparatus and simplified task for the observer brings the same general relation of values as were obtained in the first set of experiments with the pendulum. Observer B. now, as before, can more readily detect a departure from simultaneity in the illumination when departure actually is of succession rather than of motion. Observer S. again, for a part of the set—when there were single presentations—shows about equal facility with either kind of stimulus; but with double presentations shows greater ease in dealing with successsion.

Any one interested in method, and in the psychological indications arising from differences therein, will perhaps feel inclined to compare more closely the outcome by single presentation (Tables III. and IV.) and by double (Tables V. and VI.). It will be seen that Observer B. has, so far as discrete succession is concerned, a slightly greater success by the method of double presentation, and with motion he shows

a decidedly higher ratio of successes to failures when the presentation is in pairs. Observer S., when discrete sequence is offered, succeeds markedly better with pair-presentations than with single-presentations; when motion is offered, the ad-

Table VII

Observer B. Direct Vision

With Discrete Succession

Time Interval	Judgment by Single Presentation		Judgment by Double Presentation	
	Successes	Failures	Successes	Failures
20σ	13	1	11	1
15σ	10	4	10	2
10σ	10	4	8	4
5σ	6	8	6	6
Totals	39	17	35	13
Ratio of successes to failures	2.3 : 1		2.7 : 1	

With Motion

Duration	Judgment by Single Presentation		Judgment by Double Presentation	
	Successes	Failures	Successes	Failures
20σ	10	4	7	5
15σ	6	8	8	4
10σ	2	12	8	4
5σ	6	8	4	8
Totals	24	32	27	21
Ratio of successes to failures	0.75 : 1		1.3 : 1	

Table VIII

Observer S. Direct Vision

With Discrete Succession

Interval	Judgment by Single Presentation		Judgment by Double Presentation	
	Successes	Failures	Successes	Failures
20σ	12	0	14	0
15σ	9	3	10	4
10σ	5	7	11	3
5σ	6	6	11	3
Totals	32	16	46	10
Ratio of successes to failures	2 : 1		4.6 : 1	

With Motion

Duration	Judgment by Single Presentation		Judgment by Double Presentation	
	Successes	Failures	Successes	Failures
20σ	10	2	13	1
15σ	11	1	13	1
10σ	7	5	7	7
5σ	6	6	6	8
Totals	34	14	39	17
Ratio of successes to failures	2.4 : 1		2.3 : 1	

vantage for the method in which pairs of presentations occur disappears. The following tables (VII. and VIII.) in which the comparable values are isolated and set side by side and summed, will, I trust, make this more clear.

In general it would appear that a greater fineness of perception is possible by the method of paired presentation than by that of single presentation. And furthermore there is noticeable in the values obtained by the method of paired presentations a somewhat greater regularity; the number of successes or of failures keeps better step with the change in the time-interval, the correlation between the interval employed and the success of perception is closer when this method is employed than when its rival.

Experiments with Eccentric Vision.—Such was the evidence upon the main problem, so far as a foveal vision is concerned. Experiments were tried also with eccentric vision, the observer fixating with both eyes a point 20° to the left of the slit in which the illumination appeared. Tables IX.

Table IX

Experiments with Eccentric Vision

Observer B

Time	Discrete Succession		Motion	
	Successes	Failures	Successes	Failures
30σ	11	1	10	2
25σ	8	4	10	2
20σ	6	6	6	6
15σ	8	4	8	4
Totals.	33	15	34	14

Table X

Experiments with Eccentric Vision

Observer S

Time	Discrete Succession		Motion	
	Successes	Failures	Successes	Failures
30σ	4	0	4	0
25σ	3	1	4	0
20σ	11	1	11	1
15σ	9	3	9	3
10σ	9	3	6	6
5σ	9	3	4	8
Totals.	45	11	38	18

and X. show the results of these experiments. The presentations were always by pairs; the procedure was in all other respects as last described.

Under these conditions it is apparent that, with Observer B., there was almost equal success with either kind of stimulus, although the values indicate in both cases a higher threshold than with direct vision. With Observer S., there is somewhat greater facility in dealing with succession than with motion, although here the values indicate no rise of the threshold. Thus in this region as in others, succession is certainly at no disadvantage as compared with movement.

Absolute Time in the Results.—As for the absolute time-values obtained in these experiments, they run considerably lower than we have been accustomed to regard as obtaining here. It will be recalled[1] that Exner found 15σ to be the duration needed for perceiving motion; 45σ, for discrete succession. Bourdon recognized motion at 27σ; with discrete succession right judgments exceeded wrong at 20σ; the time order of succession was caught with certainty at 48σ. Weyer found a slight drag at 12σ, a 'flicker' at 25σ–30σ, clear succession at 43σ–53σ. Charpentier discerned succession at 27σ.

In the portion of the present experiments where the vision was direct and the conditions otherwise were most favorable, Observer S., even with so brief an interval as 5σ, distinguished succession from simultaneity 11 times in 14 trials (*i. e.*, 80 per cent. of the judgments were correct; *cf.* Table VI., p.) 277. B.,

[1] See pp. 263 ff. of the present paper.

when the interval was 10σ, correctly made this distinction in 10 of the 14 cases offered him (70 per cent.; *cf.* Table III., p. 275, and Table V., p. 276, where two thirds of B.'s answers were correct, with 10σ). And, again, with indirect vision, an interval of but 5σ still permitted Observer S. to give correct judgments regarding discrete succession in 9 cases out of 12 (75 per cent.; *cf.* Table X., above.

Such a lowering of the time-threshold to about one third of what usually has been considered its lowest value (to 5σ, as against 15σ), if motion be regarded as offering the finest measure of the time-threshold; or to about one ninth of its accepted value (to 5σ, as against 45σ) if succession be taken as criterion,—this lowering is to be ascribed to a combination of causes: perhaps (1) to the method of presentation, inasmuch as examples both of simultaneity and of non-simultaneity, it was known, would be offered, and the observer had simply to affirm which was which; and (2) to the instrumental arrangement,—whereby there was given an immediate juxtaposition of successive lights, yet without fusion—an arrangement analogous to the successful 'vernier-method' in experiments with space; and (3) to practice, since with each subject a far greater number of experiments was completed than has been usual in this field.

Some might feel tempted to say that the lowering of the values arose from the fact that the judgments here given were not due in the first instance to time-perception at all; that they were time-judgments, but derived from an appearance not primarily temporal—derived perhaps from intensive differences in the light at the point of junction of the two halves of the slit. I have myself tried impartially to entertain a supposition of this kind, but after serving long as an observer in the experiment the phenomenon has seemed too me clearly a time-phenomenon; has seemed to be the conscious presence or absence of precise and punctual synchronousness in the luminous slit. There appeared to me no intensive difference between a pair of lights that were exactly simultaneous and a pair in which succession was just discernible; nor is it probable, although still conceivable, that such a difference was there.

Optimal Orders or Directions.—To one other aspect of the observers' judgments attention might be called,—namely to an occasional partiality which the observers had for a particular *order* of succession, a particular *direction* of movement. If we segregate the judgments passed upon a succession actually 'above-below,' and again the judgments passed upon a succession actually 'below-above'; and likewise with motion downward and motion upward, we obtain the values set forth in Tables XI. and XII.

Table XI

Observer B

Segregation of Different Orders and Directions

Time	Succession				Motion				
	Above-below		Below-above		Downward		Upward		
	Successes	Failures	Successes	Failures	Successes	Failures	Successes	Failures	
30σ	6	0	6	0	4	2	4	2	Single presentation—direct vision.
25σ	6	0	6	0	5	1	4	2	
20σ	7	0	6	1	5	2	5	2	
15σ	4	3	6	1	4	3	2	5	
10σ	6	1	4	3	2	5	0	7	
5σ	3	4	3	4	2	5	4	3	
Total.	32	8	31	9	22	18	19	21	
20σ	6	0	5	1	2	4	5	1	Presentation in pairs—direct vision.
15σ	5	1	5	1	5	1	3	3	
10σ	4	2	4	2	4	2	4	2	
5σ	3	3	3	3	0	6	4	2	
Total.	18	6	17	7	11	13	16	8	
30σ	5	1	6	0	5	1	5	1	Presentation in pairs—indirect vision.
25σ	4	2	4	2	5	1	5	1	
20σ	3	3	3	3	2	4	4	2	
15σ	4	2	4	2	2	4	6	0	
Total.	16	8	17	7	14	10	20	4	
Grand total.	66	22	65	23	47	41	55	33	

In Table XII. (Observer S.) it will be noticed that with succession, when the order is above-below the total number of successes is 81, as against 63, when the order is reversed (below-above); and likewise with motion, when direction is downward the successes are 74, as against 58 when the direction is reversed. And this relation that appears in the grand

Table XII

Observer S

Segregation of Different Orders and Directions

Time	Succession				Motion				
	Above-below		Below-above		Downward		Upward		
	Successes	Failures	Successes	Failures	Successes	Failures	Successes	Failures	
30σ	5	1	5	1	6	0	6	0	Single presentations — direct vision.
25σ	6	0	5	1	5	1	4	2	
20σ	6	0	6	0	6	0	4	2	
15σ	6	0	3	3	6	0	5	1	
10σ	5	1	0	6	4	2	3	3	
5σ	4	2	2	4	3	3	3	3	
Total.	32	4	21	15	30	6	25	11	
20σ	7	0	7	0	6	1	7	0	Presentation by pairs—direct vision.
15σ	6	1	4	3	7	0	6	1	
10σ	6	1	5	2	5	2	2	5	
5σ	6	1	5	2	4	3	2	5	
Total.	25	3	21	7	22	6	17	11	
30σ	2	0	2	0	2	0	2	0	Presentation by pairs—indirect vision.
25σ	2	0	1	1	2	0	2	0	
20σ	5	1	6	0	6	0	5	1	
15σ	4	2	5	1	5	1	4	2	
10σ	6	0	3	3	5	1	1	5	
5σ	5	1	4	2	2	4	2	4	
Total.	24	4	21	7	22	6	16	12	
Grand total.	81	11	63	29	74	18	58	34	

totals also appears in the sub-totals obtained by each separate method and region of the retina; there is invariably greater success with the order 'above-below,' invariably greater success with the direction downward.

In Table XI. (Observer B.), the partiality is exceedingly faint, if not entirely absent; certainly absent when succession is being offered, for here the total number of right judgments when the order is 'above-below' is almost exactly the same as with the reverse order (66 as against 65) ,and the sub-totals show a like approximation. But with motion there is perhaps a slight partiality for the upward direction (55 successes as against 47 with motion downward),—a partiality, however, so phlegmatic that a mere change of method from double to single presentation is sufficient to reverse it.

Nor is the partiality confined to differences in vertical direction; it appears in experiments with the horizontal direction as well. With Observer S., in whom the difference in result for the different orders of appearance was most pronounced, supplemental experiments were tried in which the stationary slit (*i. e.*, the slit upon the front screen) lay in a horizontal instead of a vertical position, and with the rest of the apparatus so adjusted as to give illuminations of the two halves of the slit in the order 'left-right,' as well as in the order 'right-left.' And also (to answer the question whether the advantage might in any way be due to the fact that the stationary slit was a parallelogram and so permitted the half of the illumination that came from that portion of the radial slit lying nearer the center of the wheel to appear an almost infinitesimally longer period than the illumination from the more distant portion), experiments were performed with two different characters of stationary slit,—in the one case, as hitherto, with sides exactly parallel; in the other case, with sides slightly departing from this arrangement, so that,

TABLE XIII

OBSERVER S. DIRECT VISION, PAIR PRESENTATION, HORIZONTAL DIRECTION

Succession

Time	Left-right		Right-left		
	Successes	Failures	Successes	Failures	
10σ	11	1	12	0	With parallelogram slit.
5σ	6	18	19	5	
	17	19	31	5	
10σ	9	3	12	0	With radial slit.
5σ	6	6	12	0	
	15	9	24	0	
Total.	32	28	55	5	

had they been extended, they would have met at the center of the wheel (*i. e.*, a *radial* slit instead of a narrow parallelogram). Thus both halves of the slit gave absolutely equal durations of illumination. In all other respects the procedure was unchanged. Table XIII. shows the outcome of these experiments.

The advantage, which with the vertical slit was for the order 'above-below,' here reappears strong, and for the order 'right-left.' Out of 60 trials with this particular order, but 5 errors were made; whereas for a like number of trials with the opposite order 28 errors were made.

These partialities had already been noticed, as I have said, in the earlier experiments, with the pendulum; and in Table XIV. they are set forth in conjunction with those found in the later experiments.

Table XIV

Unconscious Partialities for Particular Orders and Directions

(*I. e.*, the order or direction in which there was heightened facility, or lowered threshold.)

Observer	In Case of Succession		In Case of Motion	
	Earlier Experiments (Pendulum)	Later Experiments (Wheel)	Earlier Experiments (Pendulum)	Later Experiments (Wheel)
B	above-below (slight)	no preference	upward	upward (slight)
S	above-below	above-below right-left	downward	downward

The suspicion with which I closed the earlier set of experiments, that such partiality was a mere appearance due to some aberration in the apparatus[1] was thus evidently unwarranted. The unequal facility of dealing with the rival orders and rival directions reappears in the later experiments and in the same general sense: the preference for upward motion in the earlier experiments in answered by a preference for the upward motion in the later; the preference for the succession 'above-below' is answered by a preference for this same succession. Only in B., in whom the tendency is weak at best, the partiality grows less in the later experiments, passing away entirely in the case of succession. It is thus an individual matter; for the different observers do not agree in their partialities; and of course it is not to be regarded as a conscious liking. No observer *expressed any preference* for an order or a direction. By 'partiality' is meant, then, no emotional difference, but merely a greater

[1] See p. 272.

ability of perception or of discrimination. It seems to have some similarity (though I can hardly believe that there is any causal connection between the two) to the greater aptitude for detecting a departure from linear continuity when the departure was upon the right, rather than upon the left, of the standard vertical;[1] or to the personal differences in dealing with successive shocks and successive clicks which others have found.[2] In explanation of the partialities appearing in the present experiments, I feel tempted to believe that they may be due to certain obscure habits or tricks of attention. It is probable that, other things equal, some persons are slightly more ready to attend to an object in the upper half of the visual field than in the lower; while with the others the reverse may be true. One whose attention had any such inclination would seem to have the foundation for a partiality like that of the present experiment. It is significant, in this connection, that with S., the advantage which a particular order of succession holds is shared by the corresponding direction of motion. With B., however, this is not true, at the very time when succession in the order 'above-below' had an advantage, the advantage in the region of motion lay with the direction opposed to this, lay namely with motion upward.

V. The Precise Relation between Succession and Motion

Turning now to the main question, which serves as a title to this paper, I believe we shall be more likely to succeed in answering it if we divide and attend to each division separately. (1) Is the consciousness of rapid motion (such as we have had in these experiments) intimately bound up with the consciousness of succession; or are they distinct and even independent mental processes? (2) Are these mental processes, whatever their relation may be, rightly designated as *sensations?* These are separate questions, although the an-

[1] See 'A New Determination of the Minimum Visibile,' etc., Psychological Review, VII., 429; and 'Visible Motion and the Space Threshold,' Psychological Review, IX., 433.

[2] Hamlin, 'On the Least Observable Interval,' etc., *Amer. Journ. of Psychol.*, VI. 564 ff. Drew, 'Attention,' *Amer. Journ. of Psychol.*, VII., 533 ff.

swer to one may involve or determine the answer to the other.

In answer to the preceding of these questions, one may speak first in negatives. I am inclined to believe that the facts brought out by the present experiments, in conjunction with what has been done before, very nearly annul the supposed evidence for the independence of succession and motion; at least so far as the claim for the priority of motion is concerned. For it is now apparent that the detection of motion is nicer than that of discrete succession neither on the spatial side nor yet upon the temporal. Motion cannot be discerned within space-limits too small for discriminating positions; nor within time-limits too narrow to permit the conscious distinction of instants.[1]

On the other hand, a certain claim might now be set up for the priority of discrete sequence. For upon the whole, the threshold when 'succession' was employed was lower than when there was motion. Moreover this meant not simply that an external and objective succession broke up the appearance of simultaneity more readily than did motion, but that the observers were inclined, under the conditions here arranged, and with continued practice, to give their judgment of such non-simultaneity in the form 'sequence' long before they were ready to declare it to be 'motion.' And, indeed, this seems to me in keeping with what we know from a wide variety of experiments: in keeping with the greater ease in deciding the vaguer—the more abstract—feature of an impression; for the decision as to the more definite, the more concrete, characteristics comes later and with more difficulty. Since, in contradiction to Berkeley's principle, we can tell that a pressure has changed, far more readily than we can decide whether it has become heavier or lighter;[2] and since

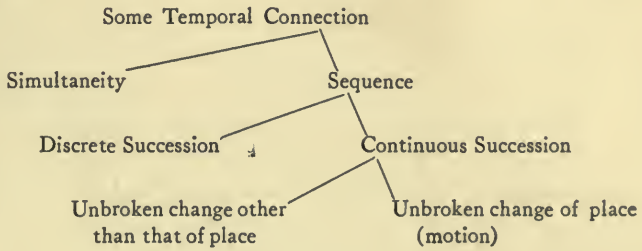
[1] Nor, it might be added, can motion be noticed at an *intensity* too weak for the detection of a motionless light. Contrary to a certain prejudice aroused by the *attention* which moving objects so readily command, a moving light, when attention is full upon it, cannot be seen at so low an intensity of stimulus, as can a stationary light. Here again the supposed sensory-advantage of motion proves ill-claimed. See 'Some Experiments on the Perception of the Movement, Color, and Direction of Lights,' etc., in the *Johns Hopkins Studies in Philosophy and Psychology*, No. 2, pp. 88 f.

[2] See 'Ueber die Wahrnehmung von Druckänderungen,' etc., in Wundt's *Philos. Studien*, XII., 534 ff., 539.

we show a like tendency in many other fields; it would not be surprising if, at bottom, it were easier to decide that simultaneity had in some general way been violated,—that the impression was in some vague form *successive*,—perhaps even without being of a succession distinctly *discrete*—than to decide whether it was succession plus those special and particular marks of *continuity both in space and time* that characterize motion. Succession means merely change; discrete succession means change with some temporal interim or cessation of the process, motion means that the change has no such temporal interim, and furthermore that there is perfect continuity, along with shift, in the space involved. But such an account implies some weakening of the claim to priority on the part of *discrete* succession; for this is almost, though not quite, as advanced a specification of succession as motion is.

The positive side of the answer to the question as to the relation of sequence and motion would therefore seem to me to be this: that motion involves as a constituent element sequence although not discrete sequence; the rather it involves sequence particularized in a way soon to be described; but in any event, that motion is the more special and advanced form of the sequence-consciousness. Yet it is the one to which the mind rushes headlong. For just as in our perception we hasten to fill out the lacunæ of impression, even at the risk of illusion; just as it is, in one sense, far easier to see *as a man* the form and color which a human being offers to the eye and color; and yet this interpretation, while most difficult to suppress, is an advance and addition to the consciousness of mere form and color—so with motion. The rapid sensory sequences which nature offers us in any one region of space are predominantly *continuous* sequences—continuous in both space and time; the flutter of leaves and of grass, the darting of birds and of insects, the quick action of eyes and lips and fingers in men about us. And so the mind becomes accustomed to this manner of interpreting all rapid change; it interprets it instinctively as *motion*. But

this precipitancy of nterpretation should deceive the psychological analyst no more in the one case than in the other. As little as we should now be inclined to say that the perception of a cow is independent of the consciousness of extensity, although most of us see cows in a field more readily than red-extensities there; so little in the end will we be inclined to regard motion, for all its greater readiness to arise in the mind, as independent of sequence or succession. Motion is different from sequence, whether discrete or continuous, but only inasmuch as it is sequence made specific, made more concrete by giving it a peculiar spatial character. The relation of those processes, so far as complexity and dependence is concerned, might be illustrated by the accompanying diagram.



When the mind is not on its guard, it rushes, on the slightest excuse, through all the intermediate stages to a conviction of movement. In that sense, the motion-interpretation is simpler; it is more natural, it is the course in which experience has drilled us. But the mind needs a more *intense* stimulation, it requires an impression whose differential marks are more pronounced, if the mind is to be, not merely more confident, but actually more accurate in detecting the real character of the change presented to it. Practiced observers therefore when confronted with faint changes such as occurred in these experiments unwittingly fall back, sooner or later, on the less commital form of judgment—that of 'succession' merely; passing on to the farther specification,—the judgment that the succession is actually of 'motion'—only when the impression has become more clear. They have become schooled out of their impulsive readiness to take all rapid

visual change (unless there are unmistakable signs to the contrary) as movement.[1]

But the fact that, on the whole, my observers reserved the judgment 'movement' for the more pronounced and definite stimulations, and usually formed only the vaguer judgment 'sequence' or 'succession' or 'some departure from simultaneity,' when the impression was approaching its liminal value,—this does not touch the farther question as to what form of stimulus will best furnish the definiteness of impression that permits even the vaguer judgment to be passed reliably. It would seem from the present experiments that with trained observers discrete succession customarily gives a much greater shock, a less mistakable violation of simultaneity, than does continuous succession (motion). A less time suffices to make discrete succession seem non-simultaneous, than is required to make motion distinct from simultaneity. At times this is not so; the just-perceptible violation of simultaneity comes equally well from either source. But the other relation is perhaps the rule. Indeed, this would seem in keeping with the general intensification and shock and start which interruptions give, and the greater ease and smoothness of continuous change.

To sum up then the answer to the first of our questions, Whether the consciousness of rapid motion is intimately bound up with the consciousness of succession, or is independent; it is this: *that the two are intimately conjoined; the judgment of motion being a farther specification (more complex, and yet easier) of the judgment of sequence. The judgment of motion has a certain priority, in the sense that it is both more complex and also more readily performed*, much as the movement of four fingers of a hand is easier, though more complex, than the movement of one finger alone. *The judgment of succession, however, has its own priority, inasmuch as it is the simpler, and requires a less lasting stimulation to evoke it.*

And the answer to this first question has perhaps already

[1] This naïve readiness is well seen in the almost inevitable impression of 'winking' which a rapid intermission of light produces. The twinkling of stars with its sense of motion is, I doubt not, three fourths motion-illusion due to rapid intensive change, the other fourth the actual shift that the light undergoes by changes of refraction.

suggested the answer I feel bound to give to the second,—the question whether the consciousness of succession or of movement (even when rapid) is to be designated as *sensation*. In dealing here with out consciousness of succession and of movement the expression 'judgment' has often been employed; and yet there is hardly any need of warning that the core-substance of succession or of motion, as they here appear, is not a mere judgment, but has a strong admixture of sensation. We *see* the succession, we *see* the movement, whatever judgments we may pass upon it. But inasmuch as the conscious succession, the conscious movement, involves a certain relating and interpreting (as I have attempted to indicate, especially in regard to motion), *it would seem preferable*—if one were to limit himself to the usual alternatives—*to regard it as a perception, rather than as either a sensation or a judgment.* The sensory impression is here grasped in its simple relations of time, it is here interpreted and felt to belong to a certain familiar type of experience. And just as we speak of the perception of a cow, and the sensation of red; so too in principle (though now the span between the bare impression and its 'form' or 'system' is almost too small for observation) we must distinguish the perception of succession (or the perception of motion) from the sensations which are its ingredients and sign. The term 'sensation,' I take it, designates a sheer abstraction, designates the 'matter' of sensory impressions as distinguished from their 'form.' If then the peculiar meaning of sensation and its distinction from perception (as a consciously formed or organized impression) is to be preserved, it seems best to regard the conscious process with which we have here been so long dealing as a perception or percept, rather than as a sensation.

Yet perhaps the negative side of the argument—that these processes are *not* mere sensations—is perhaps more convincing than the positive side, which urges that they be called 'perceptions.' For the term 'perception' covers an idea that must in time be resurveyed and subdivided. We are sadly in need of a more precise expression here, and were it not for a natural hesitation at new terms, one would feel tempted to

propose that (since no external object is here 'received' or 'taken' through the sense, or the sensation,—which is a prominent element in at least the earlier idea of perception) we designate these simple organizations of sensory 'matter' where no physical 'thing' is caught, as 'infra-percepts,' or 'æsthamorphs.' For there is here the need of marking a stage where sensation has been organized, yet not organized into a perceptive object or thing.

Summary

1. When successive lights are exactly juxtaposed spatially, motion that is just perceived does not occupy a briefer time than mere succession that is just perceived.

2. On the contrary, practiced observers can usually perceive mere succession whose time-limits are less than those of just-noticeable motion.

3. And the judgment of such observers more readily takes a form affirming mere non-simultaneity rather than motion.

4. Under the conditions here arranged, a succession of lights but 5σ apart could be discerned by the writer in 80 per cent. of the trials,—and the other observer also detected succession considerably below the limits hitherto accepted as obtaining in this region.

5. For each person, there is usually an optimal direction of succession or of movement. That is, a succession or movement in the order above-below (or the reverse) may be noticed at a rate at which the succession or motion in the opposite direction is quite imperceptible. This preferred order is not the same for all persons, and in the same individual the preferred order for succession need not be the preferred order for motion.

6. The general relations described above appear to obtain in eccentric as well as in foveal vision, although the absolute values in the two regions may differ.

7. A change of method from that of single presentations to the method of paired presentations generally brings about both a nicer and a more constant recognition of the phenomenon offered.

8. The relation between the consciousness of succession and that of motion is exceedingly intricate. The consciousness of motion is a farther 'specification' of the consciousness of succession, somewhat as the consciousness of increase or decrease of pressure is a farther specification of the consciousness of mere change of pressure. The two orders of perception are certainly distinct, although quite as certainly not independent, since the more specific apprehension always involves the less specific, but not *vice versa*. The seeing of motion is a more elaborate, and yet at the same time more facile mental act, than the seeing of mere succession. The headlong readiness to interpret all neighboring successions as *motions*, to which the world has trained the mind, can however be checked by special and cautious observation, and then the essentially simpler judgment or perception, that of mere succession, is the one that more readily arises.

9. The consciousness of rapid motion or of rapid succession is not a mere sensation, nor is it a mere judgment, nor again is it a perception in the older historic meaning of the term. It is rather a sensation or group of sensations consciously organized, and yet not organized into anything so concrete as a 'thing' or a substantial object. It therefore lies somewhere between sensation and perception, and seems to call for a special designation,—perhaps 'infra-percepts' or 'æsthamorphs.'

295

VOL. XVIII. No. 5. September, 1911.

THE PSYCHOLOGICAL REVIEW.

IMAGELESS THOUGHT[1]

BY JAMES ROWLAND ANGELL
University of Chicago

I

The problem of imageless thought has taken two main forms in current discussion. One of these has to do with the analysis of the reflective consciousness and with the exhaustive description of the contents of the mind distinguishable in such thinking. It is alleged, for example, by one considerable party of psychologists that a good deal of our thinking goes on without the presence of any discernible images at all. There is, in the second place, as a subordinate form of the problem, the doctrine that voluntary action requires for its initiation the presence of neither sensations nor images, and that it may be carried on as the consequence of a 'pure thought.' Clearly, this second form of the problem is essentially a corollary to the first form, inasmuch as the existence of such imageless thought is, in the nature of the case, a question prior to the use of such thought to control conduct.

Partly owing to a discussion carried on in the pages of the *Philosophical Review* with Professor Stout, the editor of *Mind*, and partly as the result of certain statements in the writer's 'Psychology,' his name has been involved in the controversies which have raged on this subject in contemporary literature. Up to the present time the author has held his peace in the face of the attacks to which he has been subjected by the defenders of imageless thinking. This occasion seems an appro-

[1] An address delivered at Columbia University, February 3, 1911.

priate one, however, upon which to initiate a counter attack. This course is particularly justified in view of the rapidly increasing literature on the experimental psychology of the thought processes, in connection with which the issue involved in the imageless thought doctrine is peculiarly important.

We shall address ourselves first to the question of the sheer existence of imageless thought and devote such time as may remain to a briefer consideration of the utilization of thought material of this kind in the control of voluntary action. It will perhaps serve to clear the ground in a helpful way if we turn back to the first explicit discussions of this question in our English literature of the subject, and see how the case stands. This is especially desirable in view of the persistent errors of citation, as they appear to the writer, rife among the defenders of imageless thought and even among its critics. Stout, for example, has been repeatedly cited as an advocate of imageless thought, despite his explicit disclaimer of entertaining under that rubric a doctrine such as certain of its advocates attribute to him.

The present author, in reviewing Stout's 'Analytic Psychology,' had called attention to the fact that imageless thought was put forward in that treatise as not only intelligible, but actual. Following this review, the author wrote a brief paper on the subject at issue, to which Stout himself replied in a subsequent number of the same journal.[1]

Stout's original statement had been as follows:

> "An imageless thought is no absurdity. . . . There is no absurdity in supposing a mode of presentational consciousness which is not composed of visual, auditory, tactual, and other experiences, derived from, and in some degree resembling in quality, the sensation of the special sense. . . ." ('Analytic Psychology,' Vol. I., p. 85.)

This seemed to be a sufficiently unequivocal statement and the present writer entered upon his critique, assuming that Stout really meant what he seemed to say, namely, that we have particular moments of thought in which no imagery and no sensational material is to be discovered. In the reply which Stout wrote to this article he made it quite clear that the doctrine which he advocated was in no sense

[1] *Phil. Rev.*, VI., 1897, p. 532, p. 646; Vol. VII., 1898, p. 72.

interpretable as the author had done. The ambiguity seems to attach to the word 'mode' in the passage quoted, which Stout apparently used as equivalent to the word 'phase,' whereas the reviewer had regarded it as equivalent to 'form,' or 'moment.' Stout said, for example:

"It will be said that in all such cases the presence of some specific item of sensation is necessary, and that similarly, in higher processes, the presence of some specific image is necessary. Now I do not feel sure that this is absolutely and always true. But I have no desire to contest the point. Certainly if imageless apprehension occurs otherwise than as apprehension of the meaning of a given sensation or image, it must be admitted that it plays no important part in our mental life. What I am concerned to maintain is that apprehension of meaning is a specific mode of consciousness, and that it cannot be resolved into the presence of sensation or image to which the meaning attaches. . . . I am inclined to believe that Professor Angell's criticism is largely due to his supposition that when I speak of imageless apprehension, I have in view a total state of consciousness, rather than a partial constituent of a total state, which contains as another constituent some sensation or image. What I am really concerned to deny is that when A means B for consciousness, it does so by actually recalling an image of B; and that when no image is recalled, it can mean B only in the sense of being substituted for it."

This statement would seem to be sufficiently explicit, and yet the advocates of imageless thought continue to quote Stout as if he were in reality contending for precisely that which in the passage just quoted he denies advocating. Moreover, in a passage of the 'Manual of Psychology' published a few years later he makes assurance doubly sure by the following statement which surely is not open to misinterpretation.

"An idea can no more exist without an image, than perception can exist without sensation. But the image is no more identical with the idea than sensation is identical with perception. The image is only one constituent of the idea; the other and more important constituent is the meaning which the image conveys" (p. 394).

At another point we read (p. 396):

". . . Even the highest development of conception still involves imagery, though the imagery may be, and often is, purely verbal."

In an article in *Mind*, for 1907 [p. 70 ff.], Mr. R. F. A. Hoernlé presents an admirable analysis of the connections of image, idea, and meaning, which has often been used, as have the writings of Stout, to support the imageless thought doctrine. Mr. Hoernlé, however, as I read him, holds a view which is far removed from such a position as that of Professor Woodworth, for instance, and can hardly be put into the ranks of

the true defenders of imageless thinking without the exercise of logical and ethical violence. Mr. Hoernlé is in no sense concerned to deny the presence of sensations and images in our ordinary conscious processes, but rather to point out that it is the meaning, rather than the gross substance of images or sensory materials which constitutes the important part of conscious processes. He says, for example:

"But the fact that a significant psychic whole contains as a subordinate factor sensational elements and that such a whole apparently *cannot be present in our consciousness without the help of such elements*, by no means entitles us to treat them as the only solid and substantial factor, the only objective content, as it were."

(The italics are not in the original.)

He devotes considerable attention to Professor James' account of the focus and the fringe of consciousness, in connection with which Professor James had asserted that the image or the sensation is actually the focal factor, whereas the meaning is ordinarily found in the fringe. Professor James is not on trial at the present time, and we pass over the justice or otherwise of Hoernlé's critique on this particular point. It is, however, interesting to remark that Hoernlé appears to advocate the doctrine that either image or meaning may be respectively focal or marginal. He says, nevertheless, that *sign and meaning are inseparable.* The sign, normally made up of sensational elements, is also normally subordinate, and the distinction of sign from meaning, is, in his judgment, a product of reflection. In his opinon meaning, and not sign, is ordinarily the focal thing. We regret that space does not permit us to consider more fully this particular doctrine of Hoernlé's, for there is certainly some reason to feel that meaning and image are hardly to be distinguished as being in any given case the one focal, and the other marginal. It is at least a plausible view that they come and go together, and that when we say one is marginal and the other focal, we are describing a situation in which one kind of meaning is focal and one kind of image marginal, in disregard of the fact that another type of meaning is simultaneously marginal, and another matrix of imagery is focal. At all events, the facts are not so obvious concerning this issue, as Mr. Hoernlé's account would indicate. Moreover,

there is ground for suspicion that Mr. Hoernlé shifts in his discussion between an account of logical meaning as such, and the experiencing individual's consciousness of meaning, which is often quite another thing.

In a similar way, Binet has been repeatedly cited as an unequivocal advocate of this imageless thought as a real form of conscious experience, which may be present without either sensational or imaginal thought constituents. I am disposed to believe that Binet perhaps has held this view from time to time. But the passages which were originally quoted in support of this interpretation, are altogether equivocal. He said, for example, in a treatise on the experimental study of intelligence[1] that consciousness is chiefly made up of images and words, but that thought may be an unconscious act of the spirit, which in order to become definitely conscious requires words and images. In the same treatise he said that he came upon many cases of thought in which it was not possible for his subjects to detect any imagery. It appears, however, from these citations, that at that time he was not advocating a doctrine of imageless consciouness, but rather a doctrine of sub-conscious or non-conscious intellection. In a later article which appeared conjointly with Simon in the *Année Psychologique* for 1908, p. 333 ff., and especially 339, he takes the ground that thought without image and without words is an actual phenomenon of what he calls intellectual sentiment, whatever this may mean. It appears, in any event, to connect the imageless thought process with affective aspects of mental life, as over against the purely cognitive ones. This would seem to give no comfort to a doctrine of purely intellectualistic thought content, disconnected with the ordinarily recognized elemental aspects of conscious experience.

Professor Woodworth has undoubtedly been the most persistent and unmitigated advocate of the imageless thought doctrine among American writers. Indeed, he has perhaps considered the matter from more angles of experimental study than any other one person. His statements lack nothing in

[1] 'L'Étude experimentale de L'Intelligence,' p. 108, Paris, 1903.

radical character, however convincing they may be found. His views are so well known, and particularly to this audience, that extensive quotation is hardly essential. He has from the first advocated the most extreme form of the doctrine, in maintaining that we have moments of consciousness, from which both sensory and imaginal elements are entirely excluded. Moreover, he espouses the most extreme possible view about the independence of image and meaning. He says, for example:

"An image may call up a meaning, and a meaning may equally well call up an image. The two classes of mental content differ in quality as red differs from cold, and anger from middle C. They may also differ in importance for the purposes of a given thought. Otherwise, it is hard to see any essential psychological difference between them." (*Journal of Psychology and Philosophy*, Vol. III., 1906, p. 707.)

He renders it impossible to explain away his alleged phenomena by any reduction of them to physiological or sub-conscious activities, because he asserts with great vigor that the mental states which he has in mind, involve the clearest and highest lights of conscious experience.

In his recent little monograph on the 'Experimental Psychology of the Thought Processes,' and still more recently in his 'Textbook of Psychology,' Professor Titchener has entered upon a somewhat elaborate critical evaluation of the recent germanistic movement, dealing with the analysis of the thought process, as represented in such writers as Watt, Ach, Messer, Bühler, and others. This is altogether the most systematic effort which we have thus far had, to bring together the somewhat discrepant and wholly undigested literature of the subject. Mr. Titchener occupies a relatively conservative attitude on many of the essential points at stake. Nevertheless, he is quite outspoken in his unwillingness to accept the extreme interpretations of the alleged facts such as are advocated by Bühler, and Woodworth. He says, for example, that he remains entirely unconvinced that there is any such independent thought element as Bühler [and in a somewhat different manner Woodworth] advocates, and that neither as a result of his own experimentation, nor as a result of his careful and non-partisan reading of the experimental literature, is he able to feel at all convinced that

the accounts of a knowledge process in terms of sensation and image are inadequate to account for all of the authenticated experiences.[1]

We have had a considerable number of briefer discussions in the columns of our technical journals, and one or two independent monographs, like Miller's 'Psychology of Thinking,' Dewey's 'How we Think' and Pillsbury's 'Psychology of Reasoning,' in none of which is the imageless thought doctrine openly espoused and in the first of which explicit ground is taken on the orthodox (?) side of this controversy. Pillsbury, in the *Psychological Review* for 1908 (Vol. XV., p. 158) has argued that meaning is primary and image secondary—in fact only another kind of meaning. The genetic primacy of meaning had already been advocated by Stout. Gore, in an interesting paper on meaning,[2] has advocated the doctrine suggested by certain of Dewey's positions, that the image represents the stimulus in consciousness, and meaning the response. This is to say that the image is correlative to the structure, whereas meaning is correlative to the use we make of our materials. Professor Mead and Dr. Kate Gordon have advocated similar views in occasional papers, and the present speaker, in his textbook of Psychology, has also an account in some ways closely resembling this. Mr. Thorndike has entered into the general circle of controversy with several papers, and with passages in his 'Elements of Psychology.' He, however, is mainly concerned with the question of volition, and will be considered at a later point. Under the influence of Woodworth and Thorndike, Betts has written a monograph on the Distribution and Function of Imagery [Columbia Cont. to Education, 1909, No. 26] in which he ventures to enlarge upon reports of imageless thinking, after having frankly eliminated verbal imagery from the discussion.

When one brings together all the literature of the subject in English, of which the above citations are illustrative, several distinct questions present themselves as containing the pith of the controversy. These may be formulated in a provisional way, at least, as follows:

[1] *Op. cit.*, pp. 180–182.

[2] 'Studies in Logical Theory,' John Dewey and others, p. 183.

First, is there an *aspect* of cognitive experience, which is immediately given in consciousness, distinct from sensation and image?

Second, are there *independent moments* of cognitive consciousness, in contradistinction to mere aspects of such moments, in which neither sensation nor image is to be detected?

Third, supposing them to exist, are these alleged moments of consciousness which are devoid of sensational and imaginal components, in reality automatized forms of perception or of ratiocination, which in an earlier stage would have disclosed distinct evidences of imagery or sensation? Are they, in other words, processes well along towards automatism, having originally been characterized by a much higher degree of consciousness?

Fourth, going still further than the previous question, are these alleged moments of consciousness, in reality instances of unconscious cerebration, to use a phrase more familiar to our forbears? Are they instances in which cerebral processes, for the time being excluded from direct contribution to the focus of consciousness, produce consequences which then appear in the conscious stream as the results of reflective thought? In this connection, of course, the advocates of 'split-off consciousness,' of double consciousness, are in position to allege that such phenomena have to do with dissociated foci of consciousness?

Fifth, are these alleged moments of rational thinking cases in which consciousness is monopolized by the awareness of attitudes, in which, originally at least, kinæsthetic sensory elements were conspicuous, but which, in the developed mind become, as attitudes, so habitual as to render introspection extremely difficult?

Sixth, in part equivalent to the second question and appearing as a subordinate feature of each of these problems, is the question whether meaning is a conscious element separated out and existing apart from both image and sensation.

II

With reference to the first point, I doubt whether any psychologist of repute would call in question the affirmative reply to this issue.[1] Modern writers have varied widely in the extent to which they have seen fit to emphasize this aspect, *i. e.*, the meaning aspect, of our cognitive experience, but it would be difficult to bring any satisfactory evidence to show that they disbelieve in its reality, and in the case of not a few writers, from Thos. Brown down, of whom Stout in his 'Analytic Psychology' may serve as a conspicuous example, there has been abundant emphasis on the reality of this feature of cognition.

I find it difficult, therefore, to explain the necessity which Mr. Woodworth has felt to defend so energetically the presence in perception of non-sensory elements of consciousness.[2] When he points out, for example, the peculiar manner in which we may interpret equivocal drawings as now one thing and now another, he seems to me to be stating merely the familiar doctrine of perception, without adding anything which seriously needs defense, and above all, a doctrine whose truth should not be exploited in the interests

[1] Inasmuch as the main contentions of this paper are definitely critical of the experiments and the theories of Mr. Woodworth and Mr. Thorndike among American writers, and of the Würzburg school and its critics among the Germans, to whom more explicit reference will be made below, it is only fair to myself to say that I value very highly the influence of their work in stimulating psychologists to an analysis of the thought processes keener and more searching than any previous generation has known. That I think some features of their procedure open to objection, and that I demur to their conclusions in certain essential particulars, does not at all diminish my sense of obligation for arousing us all to a new interest in problems previously too much exempted from experimental analysis. I am indebted in a personal way for such criticisms on my own published views as have enabled me to clarify to myself, and I hope to others, the convictions which I hold and the grounds upon which they rest.

The more important of the recent German monographs and papers touching the general topics at one point or another are: Ach, 'Ueber Willensthaetigkeit u. d. Denken,' 1905; Watt, 'Exp. Beitraege z. einer Theorie d. Denkens,' *Arch. f. d. ges. Psy.*, IV., 1905, p. 289; Messer, 'Exp. psychol. Untersuch. ü. d. Denken,' *Arch. f. d. ges. Psy.*, VIII., 1906, p. 1; Bühler, 'Tatsachen u. Probl. z. einer Psychol. d. Denkvorgänge, *Arch. f. d. g. Psy.*, IX., 1907, p. 297; Störring, 'Exp. Untersuch. üb. einf. Schlussprozesse,' *Arch. f. d. ges. Psych.*, XI., 1908, p. 1; Dürr, 'Ueber exp. Untersuch. d. Denkvorgänge,' *Zeitschr. f. Psy.*, XLIX., 1908, p. 313; von Aster, 'D. Psychol. Beobachtung u. exp. Unters. d. Denkvorgänge,' *Zeitschr. f. Psy.*, LXIX., 1908, p. 97.

[2] *Jour. Phil., Psy., Sc. Meth.*, IV., 1907, p. 170.

of imageless thought, a bird of very different plumage I do not know where among contemporary psychologists of standing he will find the doctrine seriously defended, that perception involves the framing in the mind of an image prior to the apprehending of the perceived object. Certainly it is not the common view. Yet it is this conception apparently, in one or other of its forms, against which he is arrayed.

When it comes to the question of terminology, to determine what names we shall assign to the various apperceptive factors of experience, it is more difficult to come to agreement, and the actual literature of our subject discloses considerable variance in usage. This difficulty appears at the very threshold in the fact that a writer like Mr. Woodworth, in his discussion of the non-sensory components of perception, is evidently assigning to the category of sensation a highly abstract and artificial significance. It must be admitted that a good deal of our structuralistic psychology has been guilty of such an abstraction. Indeed, it has rather gloried in it and defended its own excesses. But, whether such abstractions be judicious or injudicious, to use the term sensation as applicable to the sheer sensuous qualities of a sense experience, and then upon taking that step to plead earnestly for the introduction of so-called non-sensory 'elements,' is a precarious procedure, provided it can be shown, with any reasonable persuasiveness, that the elements thus called in, are quite as much sensory, as those which they are enrolled to assist: that is, that they appear quite as genuinely in the response of consciousness to peripheral stimulation of the organism. And this they certainly do. This, however, is a relatively minor issue and may be allowed for the present, to rest. We repeat, however, that such a position as Stout's, in which he alleges that every instance of cognitional consciousness contains in addition to such imagery and sensation as may be present, elements of another kind, represented by the meanings involved, this doctrine has, so far as the writer is aware, no serious opponents. Mr. Woodworth, however, while recognizing such elements would apparently maintain that they do not always accompany imagery.[1]

[1] *Jour. Phil., Psy. & Sci. Meth.*, III., 1906, p. 707.

We come, in connection with our second point to what is really the crux of the whole matter, and to the most baffling of all issues. Indeed, after going through the literature, one is left with the feeling that the case is largely reduced to mere assertion and denial, occasionally to vituperative recrimination. It seems to be largely a matter of "It is!" or "It isn't!", adorned with such adjectives as taste may dictate and capacity afford.

Competent introspectionists are arrayed on the two sides of the question, and the results which they bring in are equally unequivocal and equally dogmatic. Under such conditions the burden of evidence would seem to be with those who take the negative attitude and deny the presence of that which the other party alleges to be discernible in its own person. This is logically, no doubt, the case, but scientifically it is a safe tradition which leads to the principle that any radically new scientific element shall justify its existence by unambiguous and convincing proof. However, this is no occasion to stand upon the niceties of etiquette in the matter of proof and disproof. The practical question is, after all, whether these parties, who are divided on this issue, can find some line of approach which will bring them together in reasonable agreement. It is unthinkable that either party to the controversy is prepared to rest short of a friendly conversion of its adversaries, and our immediate problem is first to find some adequate explanation of our differences of opinion, and then, if possible, to remove the source of these differences.

The most peaceable, if not the most natural, explanation for the extreme divergence of views which we meet is that we are confronted by two radically disparate types of mental organization, to one of which a form of thought is native which to the other is substantially unknown. The other obvious hypothesis involves the inference that the introspection of one or the other of the protagonists is essentially defective. The latter alternative seems the more plausible, for it surely seems more reasonable to believe that we differ in our capacity to identify a particular kind of conscious

material, than it does to believe that we are separated from one another by the possession or lack of possession of a fundamental thought quality.

In studying the introspective literature of the subject, one feels that the differences which divide certain of the writers are largely those of mutual misunderstanding as to the precise phenomena under discussion, a misunderstanding fostered in part by wide divergences in thought processes, such as our investigations of imagery types have made familiar, but not a misunderstanding based on the presence in one individual of a form of consciousness wholly lacking in the other. There are a few writers, however, of whom Woodworth and Bühler are fair examples, for whom such an explanation seems relatively inapplicable. I do not mention the other Würzburg contributors, because I find them rather less unequivocal in expression. I judge, however, that Watt might well be added.

With scientists of their training and experience, to say nothing of some of their reagents, the charge of inaccurate introspection is likely to seem to most persons highly improbable, if not preposterous. One may hope that the time has passed when the charge of inaccuracy in such a matter need indicate any personal animus, much less any disposition to reflect upon the general scientific competency of the person charged. Nowhere is the difficulty of observation so great as in the field of controversy now under consideration, and no man has ever worked in psychology with serious intent, who has not at one time or another found himself convicted of serious error in his observational reports. So long therefore as the number of persons remains small, who have been thoroughly trained in introspective methods, and who report affirmatively upon the presence of imageless thought, it will always seem a reasonable interpretation that errors of observation are responsible for the discrepancy between their reactions and those of other trained observers. But the number of individuals who are now to be counted among those making affirmative allegations is suspiciously large to justify this interpretation, although the author is at present obliged, as

the lesser of two evils, to believe that this line of explanation is essentially correct.[1]

There are a number of conscious activities, with which it is perhaps easy to confuse the supposed imageless thought process, if one may judge by the accounts offered of this process. For example, the literature is full of evidence that in framing their conclusions at least, not a few persons have failed altogether to distinguish between the presence of objective imagery, especially of a visual character, and the presence of word imagery, whether of purely auditory character, or as more often happens, auditory-motor; or as frequently happens (and when it does happen, it creates the maximum of introspective difficulty) the presence of the suppressed enunciatory movements themselves. Again, the descriptions offered strongly suggest in some cases the presence of highly schematic and extremely evanescent imagery, whether of verbal or other character. In other cases the descriptions clearly indicate that sensory materials, including the attitudes reported by kinæsthetic sensation, are employed as the carriers of meaning, in which case the search for imagery in the proper sense is necessarily futile and predestined to failure. Finally, there are not a few instances in which the descriptive accounts make it all but certain that the actual occurrences involve essentially subconscious activities, which emerge with rational results, and which are then attributed to imageless thought. I am not at all confident that there are not other conditions in which confusion is equally explicable.

These alternative hypotheses, are, however, well known to such a psychologist as Mr. Woodworth, and he energetically rejects all as entirely inapplicable to his own case. He insists that the process at stake is no subconscious affair, and quite the contrary, is characterized by the keenest sort of consciousness. Except by inference I do not find him disclaiming in quite so convincing a manner the presence of sensory and attitudinal factors, although whenever he refers to these as in any way present, he takes particular pains to speak of them

[1] In this connection should be mentioned the paper of Dürr who took part in many of the Würzburg experiments and who writes critically of Bühler's more extreme positions. *Zeit. f. Psychol.*, XLIX., 1908, p. 313.

as irrelevant. Whether they are so unimportant as he imagines, I have no method of determining, but their presence I understand him in many instances to admit. I shall later call attention to a fact suggesting that they are not so irrelevant as they appear. The only types of imagery whose denial his statements do not seem to me wholly convincing about, are those which Mr. Colvin has recently termed mimetic, and which the present writer had previously spoken of as schematic, or symbolic. Mr. Woodworth, however, must be entirely conversant with descriptions of this type of thinking, and presumably he would have identified it, if it were present in his consciousness. Certainly it is far from my intention surreptitiously to inject any such heterodox psychological virus into his system, although I frankly doubt whether his thought is as pure as he supposes. He sometimes refers to it as 'naked' and in view of its lack of descriptive raiment, this seems to be a good term.

A naturally trustful disposition is somewhat disturbed by the absence from Mr. Woodworth's reports of detailed and accurate descriptions of his imageless equipment. He surrounds himself with a cloud of negatives, denying that it is visual or auditory or tactual, denying that it has any sensational or imaginal composition, and leaving us with a mere apotheosis of the void, so far as concerns any positive assertions. It appears to be closely related to the smile of the Cheshire cat which remains after the cat has disappeared. Of course, it may be that those of us who are organized on the simpler, old-fashioned plan of sensations and images with their meaningful aspects, may be incapable of appreciating a more accurate description of these sublimated experiences. But at least we should like to have a try at it, and we seem to get but little assistance on our way, by listening to this mere chorus of negatives, telling us what the imageless thoughts are not like. Jesting aside, it certainly seems odd that it should be impossible to do more than sit back and allege the presence of these elements, if they are so important in the structure of the thinking of these individuals. Certainly the products of their thought are not markedly less

valuable, and, when reduced to words, are not notably less lucid than those of persons frankly indebted to the use of imagery for their thought processes. It seems odd, therefore, that so little has been done, and that therefore presumably so little can be done to make clear to those of us less richly organized what kind of possession an imageless thought really is. We may be abnormally stupid.

Bühler[1] makes a serious effort of this kind, but after he has gotten through denying sensation, little is left but functional terms such as I myself should use in describing attitudes or meaning or relation.

Mr. Woodworth has, so far as I know, laid rather more stress upon the presumptive support given to his docrtine by the facts of cerebral anatomy and physiology, than any of the other writers on this subject. He has repeatedly referred to the fact, which he regards as highly significant, that so small a portion of the cerebral cortex is sensory in character. From this he draws the inference that presumably the other areas of the cortex subserve functions which are distinct from sensation, and which consequently may be expected to find counterparts in consciousness which are not of a sensory character. It is, of course true, that anatomically there are very considerable regions of the cortex which do not receive impulses directly from sense organs, but on the other hand, it is not to be forgotten that the evidence is very strongly indicative of the fact that whereas one large group of regions receives impulses from the sense organs, and other large regions are concerned with the discharge of impulses into the muscles of the body, the remaining regions serve anatomically, and presumably physiologically, to connect these sensory and motor regions with one another. Ordinarily, as regards the great mass of these interconnective, or 'association regions' as Flechsig calls them, stimulation is transmitted from sensory regions either into other sensory regions, or, as probably occurs ultimately, out into motor regions. From this point of view the system is essentially unipolar in character, and it is difficult to suppose that any cortical

[1] *Op. cit.*

portion can be wholly devoid of some sensory component in view of this fact that the sensory components are prior in the series of normal innervations to the stimulations of other regions of the cortex. However, all this type of speculation must be regarded as of extremely problematic value, whether to defend, or to attack, the view of thorough-going imageless thought—at least thought considered as strictly conscious.

Mr. Woodworth commits himself also to a view which he has expressed in more extreme language than the author recalls to have seen elsewhere, in his separation, already referred to, of meaning and image. [This concerns our sixth question.] He speaks in the most confident way of the appearance of a meaning entirely devoid of an image, and conversely of an image entirely devoid of a meaning. For example, he says: "Meaning is not felt as a relation between the image and an object, but as *the thought of the object* . . . . It (*i. e.*, meaning = thought of the object—J. R. A.) is as substantial an element of thought as the image, and there is no absurdity in the notion that it may be present alone. (*Jour. Psy. Phil. Sc. Meth.*, 1906, p. 707.) We have already quoted another passage in which he makes the radical announcement which follows. It seems altogether improbable that Mr. Woodworth can have in mind by 'meaning' just the sort of thing that some of the rest of us have, when he makes this statement:

> ". . . The two classes of mental contents differ, in quality . . . they may also differ in importance . . . otherwise it is hard to see any essential psychological difference between them." [*Loc. cit.*]

Whether he has gained any comfort from Messrs. Pillsbury and Gore, who have spoken as though image and meaning were in some sense co-terminous with one another, the author does not know, but he is disposed to believe that these gentlemen have a different doctrine in mind. To say that two psychological qualities differ from one another as 'anger from middle C' and then to polish off the statement with the assertion that it is hard to see any other essential difference between them, is a good deal like saying that black is not

white, but that otherwise they are highly similar. If it proves anything for Mr. Woodworth's doctrine, it would appear to prove too much, for it would indicate that meaning, which is for him, as it is for others, one of the essential relational features of consciousness, is itself an image in disguise. However, the present writer is too much in doubt as to Mr. Woodworth's intent in this particular of his doctrine to discuss it intelligently. In the article from the 1906 *Jour. Phil. Psy. and Sci. Meth.* (p. 708), he says, however, that probably all thinking originates in terms of meaning, but that some persons have more excitable sensorial processes and so get images. This conception of course makes the image a mere annex to the essential process. He calls it 'by-play.' Bühler, be it said (*op. cit.*) holds that his 'thoughts' are quite distinct in every way from the 'ideas' with which imagery is found connected.

If now we return for a moment to our first assumption to explain the fundamental discrepancies between our controversialists, we are obliged to accept the existence of two radically distinct types of organization for thought. There is perhaps no more *a priori* reason for refusing to make this assumption than there is for questioning the reality of race distinction on the physical side, but all the analogies which it is easy to lay hands upon in the way of organic divergences within the same species, suggest variation in the line of special development, or lack of development, in tissues which are common to all the members of the species. The case now before us, if we accept this analogy, is of a different kind. It is comparable in character to the discovery of a race of men having six fingers instead of five on each hand, or having two noses or three eyes. Mythology presents us with characters of this sort. The anatomical museums also present us with such materials, but they are recognized at once as abnormal in the latter case and imaginary in the former. They do not represent types which are perpetuated and constitute any considerable portion of the race.

Now with the best will in the world, the author does not find it possible to discover any middle ground between these

two hypotheses, provided one excludes the assumption that we are really dealing with a subconscious process or with the unconscious cerebration of the earlier writers. Woodworth and Bühler, at least, are certainly not talking about subconscious processes. Binet, on the other hand, in some of his writings, certainly has had this in mind, and has given his assent to the reality of such subconscious thought. But this alternative being put aside, and all fallacy of introspection being ruled out, there appears to be no other adequate explanation except that of the existence of two different types. As between these two necessary alternatives, the writer does not hesitate to take the former as scientifically the more conservative, and up to the present time the more justifiable. It is more reasonable to assume that the introspection of a few men, however competent, may have gone astray (perhaps as several critics have urged, *because* of their method), than to introduce an element which many men, presumably equally competent, are wholly unable to verify. Undoubtedly it is the part of an open mind and of scientific honesty to be hospitable to all such new comers in the field of scientific observation. But whenever such a new-comer arrives without a properly certified passport, it is not unfair to insist that he be detained at the frontier until he can be carefully examined, and his fitness for ultimate citizenship convincingly demonstrated. This appears, in the author's view, to be the present situation in regard to the hypothesis of imageless thought.

It is a far cry from our catalogue of points to be discussed, for we have tarried unduly long perhaps on the second of the issues raised. But this was really the central issue and the rest are of secondary significance. Our third point concerned the possibility that the alleged imageless thoughts were in point of fact automatized mental acts which in an earlier stage would have revealed distinct evidences of imagery.

Speaking for himself, the author has no question that in his own thought processes there are many occasions where thinking is carried forward by verbal imagery so highly schematized and so automatized as all but wholly to escape

identification. Moreover this schematism in the imagery is often an incident of the process of 'telescoping' to which many of the writers on this topic have called attention. In view of this fact, the author cannot refrain from strong suspicion that in the case of many of the less experienced observers, imagery of this variety may well have been overlooked. Nor is it the contention of the more thoughtful advocates of imageless thought that such schematic and compressed imagery may not serve a purpose in the conveying of the meanings of thought. On the other hand, however, we have a number of instances of observers whose reports cannot be called in question on the ground of lack of training and general professional competency. When these authorities insist that there is not a vestige of such schematic imagery, one must either accept their statement as fact and make the best of it, or resort as do the writers previously cited to a charge of introspective fallacy. Wundt, for example, alleges that the imageless thought is a consequence both of defective observation and false presupposition.[1] Titchener attributes the fallacy 'to the stimulus error,' by which he means the failure to distinguish the attributes which belong to the stimulus from those which belong to the consciousness of it.[2] An issue of this character is hardly likely to be settled merely by dogmatic affirmation and denial, which is the level at present characterizing the controversy. Unless some more crucial experiment can be discovered than has hitherto been employed, so that one party or the other can be brought to a conviction of the correctness of the view of the adversary, we must simply wait for the amassing of evidence in the hope that the accumulation of experimental results will slowly create a presumptive proof for the position of one or other of the contestants.

[1] Wundt says: "The *actus purus* of the thought experiments is no fact of observation, but simply a consequence of defective observation and false presupposition." (*Psych. Studien.*, Vol. III., 1907, p. 347.

And again: "If one abstracts from the logical reflexions of the observers, the results show that the observers have observed nothing! The thought stood as a whole clearly before consciousness. But it was disembodied, incorporeal. Here at last we have come again upon the actus purus of the scholastics." (*Ibid.*, 344–5.)

[2] Experimental Psychology of the Thought Processes, p. 145.

Our fourth consideration touching the explanation in terms of unconscious cerebration is also thrown out of court as irrevelant by most of the defenders of imageless thought. As we have seen, they allege in the most explicit way that the experiences to which they refer, so far from being non-conscious, are among the most vivid and distinct which they ever meet. This, of course, does not prevent their entertaining the hypothesis that non-conscious cerebralistic activities may produce consequences which may emerge in the field of consciousness at some later period. Binet, for example, speaks of the fact that thought itself is unconscious, but that unconscious processes exercise a directive influence over the flow of conscious states. I am not aware that any of the cohorts of imageless thought has appealed to the facts of 'split-off consciousness' to account for the phenomena involved. They seem rather to have in mind a highly explicit form of normal awareness, which however, baffles description in any ordinary terms.

Our fifth point touches the question of the possible explanation of the alleged imageless thought phenomena in terms of awareness of attitudes, especially such as involve experiences marked by the presence of kinæsthetic sensations, due to the assumption of particular bodily poses. Here again, the present author would take a strong affirmative position so far as concerns his own thinking. In many instances the closest approach which he ever secured to a state in any way strongly suggestive of the descriptions of the imageless thought proprietors, is in connection with certain reflective activities, in which there is from moment to moment an almost entire absence of describable ideational material, but with a most vivid consciousness of the directional and attitudinal kind involving vivid kinæsthetic experiences. When these experiences are met with, it is often quite impossible to find anything describably present except the awareness of an attitude of expectant strain sensorially reported, together with which there is a keen apprehension of the direction in which the thought is about to move. This is often characterized by a very definite sense of the

multifold associations of a nascent kind connected with the thought, but not rising to the focus of attention. And again, the present author would be strongly disposed to interpret the introspective deliverances of many unpracticed subjects in accordance with an explanation of this type. But we are met with so positive and heated denials of the reality of this explanation on the part of the most sophisticated of the observers among our adversaries, that we are obliged to question the adequacy of such an explanation for some at least of the experiences at issue. Here again our only lines of advance would appear to be those suggested in connection with point four.

As regards point six, few of the imageless thinkers take so extreme a view as does Mr. Woodworth in his separation of the meaning factor from the image. We have already quoted him to the effect that either image or meaning may come to consciousness entirely independent of the other. We have also remarked that this view is flatly contradicted by a number of high authorities conspicuous among whom is Stout. As in all these cases, Mr. Woodworth will brook no appeal except to fact, and in his own case, he is apparently perfectly certain that meaning and image appear in the separate and distinct way above indicated. No other view could well be tolerated by him because of the extreme form in which he defends his imageless thought. The issue reverts again, therefore, to the old question of reliability in introspective report.

Thus far I have only remarked two writers, Bühler and Woodworth, who seem to adhere to this extreme formula and Bühler's conception of his 'Gedanken' is such as to render this formulation misleading as applied to him. Binet's first statements about the matter were certainly couched with a view to recognizing the fact of unconscious cerebral activities rather than the presence of a strictly conscious experience devoid of sensational content. His later accounts have, as already indicated, introduced the notion of the intellectual sentiment or feeling, a suggestion which the present writer can partially sympathize with in so far as it indicates the presence of

thought processes in which the strictly cognitive elements are submerged under affective attributes. The difficulty of description might well connect itself with this affective, non-cognitive character. Miss Calkins has expressed herself in a way which confirms my own reading. She says:[1]

"The writer of this paper frankly deprecates the tendency of certain psychologists —of Stout, Bühler and Woodworth, for example—to insist that the occurrence of imageless thought has been proved. . . . What is abundantly proved is that along with imagery, and often in the focus of attention, when one compares and reasons and recognizes, are elements neither sensational nor affective."[2]

The author, together with many other psychologists, after the most painstaking efforts to ascertain what actually occurs in his own experience, is wholly unable to confirm the appearance in consciousness of any such meaningless image, or of any such imageless or sensationless meaning. For reasons that no doubt have grown out of the similar observations of a long line of psychologists and logicians, the general theory has arisen which alleges the invariable interconnection of these two phases of mental life. To suppose that one can occur without the other is utterly to destroy the entire foundation on which rests the theory advocated by most of us concerning cognitive operations. Doubtless each of us would promptly abandon the theory provided we were confronted by convincing evidence of the fallacy of the data upon which it rests. But until we can secure something like unanimity of competent introspective opinion on the matter, we can hardly be asked to throw overboard our hard-won convictions, which equally with those of our opponents are based upon what we believe to be unimpeachable facts. A meaningless image is to the present writer not only a thing never experienced, but also a thing in the nature of the case ridiculous, a physical object free from the attribute of gravity, a light devoid of wave-length. In the same way and for the same reasons an imageless or sensationless meaning seems an impossible as well as an unexperienced event.

If we accept Woodworth's pure thoughts we commit ourselves to a belief in the prodigality of nature such as has not

[1] *Am. Jour. Psych.*, Vol. XX., 1909, p. 277.

[2] For reasons which the present writer has made clear in a previous paragraph, the inclusion of Stout in this list appears to be unwarranted.

generally been entertained in modern times. I do not find that he assigns in any convincing manner, a function to these pure thoughts, essentially different from that subserved by ideas of the ordinary variety, *i. e.*, meaningful images of objects or words or acts. Evidently, unless this can be done, we are invited to suppose that we have two generically different thought materials to accomplish one and the same result. Clearly this *may* be true. But it flies in the face of well-grounded prejudice and tradition to admit it. Possibly Mr. Woodworth calls in question the doctrine that ordinary ideas operate as we have always supposed. Certain of his statements seem to lend themselves to such interpretation.

Relevant to this point it may be added that so far as I can discern, the appearance of imageless thoughts is not connected with any specifically assignable kind of situation. They may appear on one occasion only to have their places taken by ordinary ideas on another similar occasion. This seems at all events to be the experience of many of Mr. Woodworth's subjects. They appear, therefore, to be somewhat sporadic and irregular phenomena, which is certainly not what we might expect, if they play so important a part in thinking as their defenders would have us believe. So long as they disclose vagaries of this kind, we may justly demand and wait for their further analysis before accepting them.

Bühler takes rather different ground on this issue. He apparently regards his Gedanken as serving a different and higher function than ideas. Time fails me for a careful analysis of his contentions on this point, but it is perhaps not unfair to say that they have received very severe criticism from a number of competent psychologists. Wundt, as will be remembered, refers to the Gedanken as constituting a return to the *actus purus* of the scholastics. Whether this be scholasticism or not, the view which regards ideas and 'pure thoughts' as subserving essentially different functions, appears to be much better entrenched logically, than one which more nearly identifies their properties. I do not find Bühler's citation of fact anywise conclusive as to his theory,

but merely as theory, it is intelligible and defensible. It smacks, nevertheless of the Kantian Reason.

III

We turn next to a brief survey of the evidence concerning the part played by sensory and imaginal elements in the control of our movements. Here again, Mr. Woodworth has been among the most active critics of what he calls the orthodox view, and he has contributed not a little of the most interesting experimental evidence bearing on this issue. His colleague, Mr. Thorndike, has also been a stalwart advocate of essentially the same views as those advanced by Mr. Woodworth. On the fundamental issue as to whether or not we possess the imageless thought for which these authors stand sponsor, there is little to be added to what we have already said. But they bring forward in the case of voluntary control a number of considerations which do not figure in the discussion of reflective and constructive thinking. A few notes on these additional points may therefore be permitted.

Mr. Thorndike, for example,[1] alleges that in willing *not* to make a movement we frequently find present the resident and remote sensations caused by the movement itself (or the images of these sensations), which, according to the orthodox theory, as he interprets it, ought to be used *only* for the willing *to* make the movement. Therefore he infers that these sensations or images must be irrelevant to the actual production of the movement.

This case when it arises, instead of affording a difficulty, would seem to constitute a peculiarly telling confirmation of the importance of the sensations and images concerned. To will *not* to make a movement is in so far to have that movement in mind. If the mere act of this negative willing itself reveals these sensations and images present, when all that is required is the identifying of the movement to be inhibited, it would seem to offer strong presumptive evidence of their essential significance for executing the act to which they properly belong. In the particular case cited, they are ir-

[1] *Journal of Philosophy, Psychology and Scientific Methods*, Vol. IV., 1907, p. 40.

relevant, except as reliable representatives of the movement to be estopped, and in any event I do not understand Mr. Thorndike to allege that they are always present on such occasions.

Again, Mr. Thorndike writes that we cannot well possess images, whose resident sensations are not obtainable. He cites those of willing to move the eye smoothly across a line of print, and urges that because we now know that the eye does not move smoothly, but moves by jerks, we could never really have willed this act in the manner alleged by the orthodox view.

It may certainly be admitted as truistic that we cannot obtain images of sensations which we cannot experience. But so far as concerns controlling eye movements this fact appears to be a trifle irrelevant. It may well be in the case of eye-movement, which has so many reflex factors involved, that the resident sensations and images play an insignificant part in motor control. At least, this may easily be the case, if resident be taken to mean kinæsthetic and be set over against retinal factors. It does not appear, however, just what bearing this consideration has on the fact that a voluntary movement proves under scientific scrutiny not to be precisely what common sense has supposed. It requires some kind of 'cue' under its new guise, as well as under its former one. Whether this cue be sensory or imaginal, or consists in one of the pure thoughts, is a matter for further determination. The argument appears to have force only against the theory that voluntary control always involves the employment of imagery which reinstates strictly kinæsthetic sensations, a theory whose proof ought not to be demanded of the believers in the invariable presence of sensory or imaginal material of some kind at the outset of voluntary movement.

Again, Mr. Thorndike writes that it requires too much time to get the image of each movement in a series of co-ordinated movements, such as those of speech, or writing, or the playing of a musical instrument.[1] I have not been able to discover any orthodox (!) psychologist, who has made

[1] Burnett enlarges upon this point in a paper reporting experimental observations. 'Studies in Phil. and Psychol.'; Garman Commem. Volume, p. 393.

statements reasonably interpretable as this assertion requires in order to have point, but I am quite prepared to believe that such statements are in existence. Certainly the view which has commonly been held by writers like James, for example, has been that a single cue may be adequate to release a long series of coordinated movements, provided only that these acts have been built up in some habitual manner, so that they are more or less automatized. Relative to this point, the author may remark that a number of experiments carried on in his own laboratory have revealed cases in which it has been possible to secure imagery very much more rapidly than the movements could be executed expressive of the imagery.

In the experimentation which Mr. Woodworth and his students have carried out, or which Mr. Thorndike has undertaken, there has been a crucial defect in method in the writer's judgment, which he has more than once pointed out. It is utterly unsound as a matter of method to give subjects verbal instructions concerning the movements which they are to make, and then to draw a negative inference concerning the part played by sensory and imaginal material, because, under such conditions, a certain percentage of these persons detect no imagery in the execution of the act a little later on. In the verbal cue, in this case, we find all that is needed to give precisely that 'set' to the nervous system, which Mr. Woodworth has himself so tellingly described. But the 'set' itself is occasioned by the sensorially apprehended directions. The author has participated in a long series of experiments directed to the analysis of this movement consciousness. Neither in himself nor in the many students who have served as subjects for these experiments, has there ever been a scintilla of real evidence for the initiation or control of a voluntary movement entirely without sensory or imaginal supervision. This control has often been found emanating from factors which ordinary introspection would have regarded as entirely irrelevant. For example, the control of the writing process has in certain of our subjects shown itself as dependent to a very marked degree on the sound made by the pen as it moves over

the surface of the paper.[1] Ordinarily this experience is regarded as constituting a mere annoyance and as in no sense an essential part of the control. Some of Mr. Woodworth's irrelevant conscious facts may have a similar unsuspected function.

In most of the experiments which these authors have relied upon to make their case, they have used relatively habitual forms of action, in which only a slight cue is in any case necessary to precipitate the act. In the other type of case which they cite, where a relatively new coordination is brought under control, the evidence seems to the writer perfectly convincing as regards the presence in an essential way of sensory or imaginal factors. For example, to look at the foot, or to attend to sensations from it, while trying to gain control over the muscles of the toes, is certainly in no sense to produce a situation in which sensory direction is entirely wanting.[2]

There is, here again, as in connection with the topic of reflective thought, a tendency to magnify unduly those features of other authors' writings, which they feel confirm their own views. This is no doubt an entirely unintentional over-emphasis on one side of the case. Bair, for example, whose work has sometimes been adduced in support of this view of the pure thought control over movement, makes it perfectly obvious as I read him, that in his experiments, sensory factors were repeatedly found indispensable to securing control over the muscles of the ear.[3] The author has already made quotations from Stout, which show how different his view is from that held by the gentlemen now under discussion. Of him Mr. Woodworth writes:

"Some authors, Stout, Binet, and recently Bühler, boldly assert the existence of imageless thought." (*Journal of Philosophy, Psychology, and Scientific Methods*, Vol. III., 1906, p. 701.)

One does not like to call attention in a spirit of carping criticism to matters of this kind, but if the appeal is in any

[1] Cf. Downey, 'Control Processes in Modified Handwriting,' *Psychol. Rev. Monog.*, Vol. IX., 1908, Whole No. 37.

[2] Cf. Woodworth, 'Le Mouvement,' p. 330.

[3] Psy. Rev., 1901, Vol. VIII., p. 476.

way to be made to the authority of opinion, it is fair that this opinion should be ranged where it belongs.

On the whole, the author is much more disposed to be dogmatic about the case of voluntary control, than about the case of reflective thinking. His own experience, and that of a considerable number of expert persons, who have worked on the problem in his own laboratory and elsewhere, leads him to utter skepticism of the control of voluntary movement, without sensational or imaginal factors. Moreover, there is good indirect reason why such control should not occur, although evidence of this type, if unsupported by more strictly empirical evidence, would have no especial significance. But if one thing stands out more clearly than another in the general contributions of comparative psychology, to our general theory, it is that the original reactions of animals are made in response to sensory stimulations, and that the primal significance of the reactions is to be found in the adjustment which they represent to the stimulating objects. Memory, when it makes its appearance, also finds its significance in the guidance of these movements of reaction, and it is memory of the sensory incentives and consequences—not memory of the purely extra-sensorial kind. If, at a later stage of development, thought processes are developed, which have no direct dependence upon, nor connection with, the world of sensory experience, such thought processes must have only a very secondary reference to the control of ordinary muscular activities. But, as said above, considerations of this kind are of altogether secondary consequence as compared with the direct introspective evidence gained under experimental conditions. For himself, the author is perfectly clear that these control conditions are always of a sensory or imaginal character. But it needs to be repeated that for him neither sensation nor image as such is to be regarded as isolated from the correlative fact of meaning, and meaning is not for him in any sense equivalent to the mere sensuous content of the mental state, whether it be dominantly conditioned by peripheral or central stimulation.

Finally, then, I find the doctrine of imageless thought open to suspicion on the following points:

1. The method of its experimental investigation is at least not wholly satisfactory in meeting the demands of ordinary experimental procedure. This is true, both as regards the problem of reflective consciousness, and that of voluntary muscular control.

2. Imageless thought seems with many observers to be a sporadic and occasional phenomenon. Its appearance is not in their cases invariably connected with any special kind of situation. It consequently lacks one usual characteristic of well established thought forms, and may therefore well be regarded a trifle askance.

3. Unless purely functional and logical terms be used, it seems almost impossible to describe it, save in negative terms. This suggests either that the analysis is not yet complete, or that the thing analyzed is not really a content of consciousness.

4. If Mr. Woodworth's variety be accepted as genuine, we must apparently recognize two generically different kinds of thought material to serve one general function. This will be at variance with our conceptions of the parsimony of nature. Mr. Woodworth's effort to exhibit the two forms as respectively primary and secondary to one another, will scarcely serve to avoid the factual difficulties even though it afford a logical solution. Meaning and imagery differ from one another in essential function not as does one form of imagery from another form, merely in quality. If we accept Bühler's conception, we have ideas limited in their function in a manner utterly at variance with ordinary opinion.

5. There are many well recognized conscious states which may obviously be readily confused with imageless thought. The consciousness of attitude, springing out of very primitive physiological attitudes, is an important case in point.

6. The presence of interpretative factors in perception gives no real comfort to belief in imageless thinking.

My own conclusion is that at present, the only demonstrable imageless thought is subconscious, and so primarily a matter of cerebralistic physiology. Even this would be imaginal, if it got above the limen. But I shall try to keep an open mind.

THE SYSTEM OF HABITS AND THE SYSTEM OF IDEAS

BY ROBERT MacDOUGALL

New York University

The history of the individual mind presents, from the outset, a field of complex phenomena; it has many phases, in each of which developmental changes are occurring; with the surrounding world it has manifold points of contact; and the quantitative contents and forms of organizations by which it is characterized reflect the variety and complexity of the world in which it exists.

The student's direct approach to this complex life is beset by many difficulties. To picture it in its fulness is impossible; to combine the essential features of each phase of change in such a way as to give a view of the mind's constitution at any point in its history, is a task of scarcely less difficulty. In the face of these obstacles the psychologist has usually fallen back upon one of two procedures,—either he has considered the general form of the changes under way, as when the rate at which variation occurs is studied, and the accelerations or retardations in development are indicated; or he has selected some individual phase of change for study, as when the establishment of motor coördination is traced or the development of speech followed. This work is indispensable to a full description of mental development, and each such study finds its place in the final system of genetic psychology.

But in all genetic study,—in the description of the mind's development equally with that of organic genesis—there is more to be done than to determine the general formal changes which the subject matter presents, and to trace the development of its various constitutive functions. These studies, indeed, presuppose another form of analysis as their logical antecedent: for in every organic system which has historical and not merely logical reality, a unity of constitution is to

be predicted. It is this assumption which underlies all elementary analysis, upon the systematic completeness of which rests the success of explanatory science generally. The assumption of a single constitutive unit and a common type of change characterizes the procedure of historical sciences at large and constitutes the ground-work for all subsequent treatment of a specialized character.

In the study of mental development, as in all other genetic science, this preliminary analysis must be undertaken, and the complex of mental changes treated in terms of the units which it yields. The student of genetic psychology thus sets out from the assumption that the qualitatively various modifications which the life of the individual mind presents are the complex products of a change the units of which are fundamentally similar throughout the whole series of phases. His first task, therefore, is to formulate these constitutive factors and to explain the system of changes in their terms.

The general character of mental development may be described as adaptation. At all stages and in every phase of its activity the change from the earlier to the later form is a reconstruction which tends to establish more harmonious relations between the individual and his environment, to restore a lost equilibrium between the forces which act upon the mind and its own responses. Adaptive reconstruction constitutes the general form of change, whether the origin of determination be conceived as lying in the environment and producing a simple adaptive modification, or the element of initiative be considered, in the utilization of materials for ideal ends. Such adaptation is incessantly renewed so long as the individual continues to live. That at one stage it leads to more systematic complexity in the reaction to stimulation, and at another stage results in a narrowing of the range of reactions,—that at one time it is constructive and progressive and at another involutionary and conservative, is a question of the special form of readjustment which occurs, and does not affect the general character of the process.

Adaptation involves two factors, a form of response already elaborated and an action tending to modify the adjust-

ment in conformity with a variation in the system of stimuli. The former represents the level of adaptation already attained by the organism, the latter represents the increment of advance in which the fact of development consists. The first of these two factors we call Habit, the second Accommodation. Habit constitutes the response of the organism to its environment in so far as the system of stimuli possesses permanence in the course of experience; accommodation constitutes the organism's response to variations appearing within this system of stimuli.

In so far as the variation thus introduced becomes a constituent of the system of stimuli, this means simply the formation of a new habit, the development of a fixed type of response arising through a modification of those forms of reaction which previously existed, and replacing them in the economy of the mental life. In so far as concerns its outcome, accommodation thus adds nothing new, nothing which is not fully represented in the habit-system from which the process of reconstruction took its start. The movement of development is a circular one, from preëxisting habit through accommodation to later modified habit. The terms habit and accommodation, therefore, do not serve to define the opposition which is here in question, since they have application primarily to the objective reference of the mind's attitudes, and the relations which it bears to the system of outer reality.

From the psychologist's standpoint it is the immediate conscious reaction provoked by such variations in the system of stimuli as do not arouse an already established adaptive response which must be opposed to habit and called its correlate in the system of factors constituting accommodation. This immediate reaction we call attention to the stimulus. Instead of provoking an habitual response it arouses an awareness of its own quality and of the relations in which it stands. The nature of the stimulus is thrown up in consciousness, as upon a screen, and while, in such cases, the immediate teleological response is characteristically lacking, it is through the development in a systematic consciousness of the associative complex in which the stimulus stands that the individual is led

to that later reconstructed type of reaction which constituted the fact of accommodation. To habit is thus to be opposed attention, discriminative and selective, and the systematic consciousness to which it leads. Of this selective consciousness accommodation is a function, and from it derives the whole succession of modifications which constitutes the course of mental development.

The two constituents of mental development may thus be described as the system of habits and the system of ideas. It is the progressive organization of a system of habits and the continuous elaboration of a system of ideas. Each of these factors has a specific function and a continuous history in mental development. The system of habits represents the organism's response to the permanent aspect of the environment; the system of ideas represents its response to the variational aspect. Wherever the system of stimulations presents recurrence, the organism responds with a habit; wherever that system presents novelty the organism responds with an idea.

At every stage of its history these two factors are involved in the mind's activity. There is no consciousness which is not determined by preëxisting habit, nor is there any habit which is not subject to modification through the consciousness which accompanies it. On the other hand, the relation of these two factors determines the character of any experience; and their concomitant variations mark the range of activities by which the mental field is characterized. In those provinces within which the relations between the organism and its environment are highly defined and maintained with relative continuity, the points at which consciousness is aroused are few and unimportant. The element of habit constitutes the dominant feature of the situation, and awareness of the qualitative character of the stimulus falls to its lowest ebb. Where, on the contrary, such relations are ill-defined and subject to frequent and profound alterations, a fixed type of response does not supervene, but attentive consciousness attains its greatest acuteness.

These two constituent factors of development appear in

every field of mental activity, and throughout its range maintain their fundamental significance unmodified. The function of genetic psychology is to define the place and contribution of each factor in any given phase of mental activity, and to trace the evolution of the organic system to which the two elements lead in their respective spheres.

Thus in the description of the system of perceptual and of conceptual objects alike, both of these factors are found indispensable. The response to each object of perception involves habit, since reaction to such stimuli depends upon the recurrence of typical sensation complexes in the individual's experience, the momentum of habituation in the reaction varying with the fixity of these complexes and the frequency of their recurrence. In so far as this element of routine prevails, the adaptive reaction provoked by the stimulus is automatic and unaccompanied by consciousness. In so far as the field within which the stimulus lies is subject to fluctuations, and the relations between it and the organism are incessantly disturbed, the reception of the stimulus and the reaction to which it leads will be pervaded by consciousness.

It is sufficiently obvious that the system of objects which constitute the field of perception will not become the source of a series of purely automatic adaptations. The calling forth of a wholly unconscious and habitual response will be approached only in cases where a great simplicity in the stimulus obtains, and where a high degree of uniformity in the conditions of its occurrence is possible,—as in certain mechanized signals of rhythmical recurrence with which an unvarying response is connected. If this type of reaction developed in connection with the general system of stimuli which the sensible world presents, the individual would constantly be involved in disastrous situations through variations in the concomitants of the particular stimulus which conditioned his response. There is perhaps no conceivable situation for which a purely automatic reaction constitutes a sufficient adaptation, since from none is the possibility of significant variation at any moment absent. The reactions

connected with even the most familiar objects are subject to incessant subtle modifications; and the possibility of these modifications must at every instant be taken into account in the formulation of the response.

This potential variability in the stimulus conditions what we call attention to its character at each recurrence. Perception, in a word, represents the margin of variability in the field of the stimulus. The response which the individual makes to sensible objects thus involves both attention and adaptive movement; and its elaboration gives rise on the one hand to a system of habits and on the other to a system of ideas.

In the field of conceptual objects the same phases appear. This is perhaps best illustrated by the form of the individual's reaction upon the familiar terms of speech. The response to significant speech involves the element of habit in so far as each word provokes an unreflective response in terms of the adaptation, or meaning, which it represents. The warning may thus be acted upon without distinct awareness of the words which have prompted the action. It is only when meaning is doubtful, when words are ambiguous or construction obscure, that attention is directed to the form of expression, and the margin of variability in the stimulus is developed in a critical review of the possible applications of the term.

But reflection shows that grammatical construction, at least, is always variable; and that habituation can reach a high degree of development only in connection with common words and their uses, or at most with certain simple and unvarying constructions which are in frequent use. Such are the current verbal coin of polite intercourse, the forms of which are stereotyped and become devoid of specific significance, and, on the part of user and hearer alike, lapse into practically complete automatism. In the case of all ordinary human intercourse, significant speech is essentially mobile and variable. Every element has a meaning of its own, which constitutes, in a sense, the core of the whole system of meanings connected with it; but as an element in a sentence it has a meaning which is defined only

by the context in which it is found, and cannot in any way be determined by its signification in isolation. Even when used without context it is rarely, if ever, that a term is univocal, and that it may be responded to by a single unvarying reaction.

In any case, then, response to the forms of human speech involves incessant attention which must continue throughout its course. The development of the margin of variability ceases only when the expression is brought to a close, since the meaning is constructively developed, as the terms are apprehended in succession, by a progressive modification of the whole preceding context. Every syntactic form is thus typically unique, with the exception, noted above, of such expressions as have become stereotyped; so that it is logically inconceivable that an automatic response should ever be established in connection with it. It is this character which makes human speech so much more stimulating than the perception of the sensible world. The latter approximates the values of speech as an ideal stimulus only in those extreme cases which may be termed crises, namely such as call for immediate and difficult coördination, danger, personal encounters, the control of active and variable forces, and the like. In the apprehension of human speech the margin of variability being greater and of more continuous importance, the system of ideas attains a higher functional development than that which it presents in connection with the perception of the external world. Speech provokes an incessant, lively and variable play of ideas which is the necessary consequence of the fact that from its very nature speech cannot indicate its object intuitively, but must make it known through a system of discursive symbols.

A similar quality appears in the relations which may exist between any mental image, or any concept, and the context with which it is connected in individual thought. The connections linking a series of ideas which successively occupy the mind may be so simple and variable that each suggests the next in an immediate and inevitable manner and the last of the series may lead to specific reaction without any

reflective consciousness concerning the internal constitution of the ideal system by which its occurrence was mediated. Only the terms of the series, as the event is recalled in after-experience, seem to have been in mind, the intermediate elements, whether one or many, having apparently dropped out of existence. It is not necessary, in such cases, to deny that mental activity of any kind has occurred, but if the occurrence of a succession of ideas be granted, it has conformed so closely to the habit type that attention to the constituent elements has fallen to its lowest ebb, and the margin of variability which conditions consciousness is practically negligible.

Such successions are possible only when the order of associations is extremely familiar, when thought is following some thoroughly trodden path. In every mind such idiosyncratic forms are developed, and all permanent social life fosters the establishment of common ideal types which tend inevitably to the form of habit-modes in popular usage. Such ideas are received uncritically and provoke immediately certain specific mental attitudes in relation to their object which also commonly prompt stereotyped communal expressions. Every human group owns such authoritative conceptions,—ideas and symbols made sacred by religious or political or moral sanction—which by common consent have been raised above the stage of debateableness and made sacrosanct in the thought of the multitude. These conceptions constitute the idols of the forum and of the market-place, which drug men's minds instead of stimulating them. They silence thought by demanding an immediate emotional or practical response of an invariable type. Reflection has been interdicted in connection with concepts which have reached this status and criticism is anathematized. They thus become, as in the case of much of the technical terminology of sectarian religions, merely cant phrases, employed not to stimulate the development of meaning but solely to provoke a typical habitual response, namely the specific mood or act which tradition has connected with the verbal formulation.

In this system of ideal conceptions, and in the understand-

ing of human speech as well, the function of habit is the same as that which it fulfills in the field of perception and motor reaction. It represents the system of fixed and permanent relations which finds place within each province of mental activity. These highly defined and recurrent aspects of the system of conceptions constitute the basis of rationality in our thinking. Repetition is essential to the organization of the field of consciousness as variation is to its preservation. In proportion as the relations among the mind's objects are ascertained and made familiar through repetition, they cease to occupy the attention. In the mind's procedure they still have a place, but they are neither reflectively formulated on the occasions of their employment, nor do they detain the mind at their occurrence and thus become the center of a new associative system. Conception and equation, formula and deduction follow one another in a typical habit series when they have thus reached full definition and attained final familiarity for the mind which employs them.

The system of relations represented in the multiplication table affords an illustration of this process of habituation. The relations here are simple and invariable, and while the establishment of the system in mind may involve sustained effort and a high degree of selective attention, the tabulation of this series of relations and the study of them by the individual has fulfilled its object only when the mind has been so thoroughly familiarized with the whole system that the product of any two numbers arises immediately and unreflectively upon the presentation of the numbers in question. In the final stage of this development there is no multiplying process interposed between the thought of the numbers and the thought of their product, to which the thinker can point. The procedure has been so mechanized that in the case of the expert a series of such simple operations may be performed without interrupting the continuity of attention to other objects of thought.

Just as in the case of human speech it is essential that the fundamental meanings should be mastered in such a degree that their signification needs not to be established anew on the

occasion of each recurrence, so in the general system of concepts by means of which the mind works, it is indispensible that group after group of formulations should thus be automatized if the mind is to advance in its task of reducing the phenomena of experience to logical order through the application to them of an ascending series of ideal conceptions.

In all phases of the mind's activity, whether it be motor co-ordination, or perception of the external world, or associative thought, the same phenomena are presented. At any stage in its development there is given an existing system of habit-modes which is under continuous extension and organization through a process of selection and reconstruction. The system of existing habit-modes we may call the individual mind; for the latter term is commonly employed to denote the specific characters of the individual,—those mental features which, though they undergo change, present at any time such definite forms that they constitute a synthetic type by which the individual may be recognized and described. The selective and reconstructive modification is the function of individual consciousness, which arises at those points where equilibrium has been disturbed, and mediates the new form of reaction which is in process of formulation. The interaction of individual consciousness and individual mind constitutes individual life, which is a process of continuous re-adaptation in a cyclical type of change, wherein, through the disturbance of equilibrium between an older settled habit and the environment to which it is adapted, an activity of discriminative and selective consciousness is initiated, leading to the establishment of new habit-modes through modification and re-organization of the old in an endless succession.

In this whole process the terminus to which the reconstructive activity tends is unconscious automatism. In its final phase it is an adaptive reaction, in the form of some specific movement or bodily attitude: it is never a system of ideas. The system of ideas, whether feelings of bodily condition, perception of external objects, mental images, or conceptual forms, is that phase of the mind's existence which mediates the formation of habits. The system of ideas cannot

itself exhibit habit; just in so far as habit supervenes the ideal content of experience declines. It is thus not proper to say that the system of ideas as well as the system of movements is subject to habituation. On the contrary, the system of ideas exists in so far, and only in so far, as the conditions of habit-forming fail within any given field of experience. The system of ideas represents the margin of variability in the world of experience at large. It stands for those elements and aspects which are not yet coördinated with, and capable of immediately provoking, appropriate adaptive reactions. The system of ideas thus reflects the outer circle of environmental conditions, the world of problematic relations and unsolved adaptations. It is the field of doubt and hesitation, of stress and struggle, the field within which, though the object of the will has already been formulated, the means to its elaboration have not yet found definition, and the mind is occupied with an incessant vivid representation of the alternative but still dubious courses which the situation suggests.

The vividness of consciousness is intensified wherever the form of habit suffers disturbance, as well as where it is primitively lacking. In the child's pristine experience every detail of those events which in later years we call hum-drum is eagerly welcomed; and if the sense of living experience decline with age, it is because the extension of habit in life has not been accompanied uniformly by a correlative development of interest in novel forms of stimulus. It is conceivable that the system of organized habits should approach so near to the confines of experience as to leave only a negligible element of ideal activity, confining the individual in a narrow round of routine, in which his dull and brutish soul is illuminated by only an inconstant gleam of speculation. It is conceivable also, that in a fortunately born nature the order of experience should remain so full of possible alternatives that not even the most trivial occurrence has completely lost suggestiveness, so that an aura of serious or whimsical speculation surrounds it in the fulgurating mind. Such individuals never grow old, we say; and their youthfulness lies just in this freshness of vision which enables them to avoid

the shackles of intellectual habit, and to maintain a child's originality in their treatment of the increasingly complex phenomena which the course of experience presents.

Duller minds are touched by this deepening and quickening of existence wherever their habitual routine is disturbed, as when a hitherto unconsidered doubt is cast upon some settled belief, or a familiar fact is presented in a novel but consistent way. Whether the sharpened sense of experience be found exhilarating or poignant will depend upon the specific direction of departure from the fundamental habits of life. But, in both cases alike, the experience is characterized by the sudden flooding of a field of organized and habitual attitudes with an acute and vibrant consciousness.

The normal relation of these two systems of factors in mental development may be stated briefly. The system of habits gives to ideal activity its point of origin and its direction: the system of ideas gives to habit a telic value, and maintains its commensurability with an enlarging environment. Without habit experience would be an irrational chaos; without ideas it would have no existence,—for experience is not posited of an organism in virtue of adaptation in general, but only in so far as adaptation constitutes an ideal process determined by a specific aim and a sense of value. Normal development in the individual mind, therefore, is that process of change which leads towards a more complex synthesis of habit-modes and a widened ideal horizon; and the highest type of self is that in which a life of the most intense intellectual activity finds at once its basis and its object in the fullest organization of experience in terms of significant reactions of the will.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF CALIFORNIA

XVI. Temporal and Accentual Rhythm

BY WARNER BROWN

Perhaps the only undisputed characteristic of rhythm is the impression of regularity which it occasions. Some hold that this impression arises from the regular recurrence, in time, of certain features of the rhythmic series; others claim that the regularity resides in the structure of the elements composing the series; but in either case some regularity is admitted. A rhythm lacking regularity in its structure and failing in the regular repetition of its elements would be no rhythm.

I

The mere repetition of a single undifferentiated movement or sound does not constitute a rhythmic series, properly speaking, and yet such a series offers one of the most satisfactory approaches to the more truly rhythmic forms. Movements either with the voice or by tapping, are easily recorded by the kymograph, and may be measured and studied at leisure. On that account a series of movements can be analyzed to better advantage than a series of sounds.

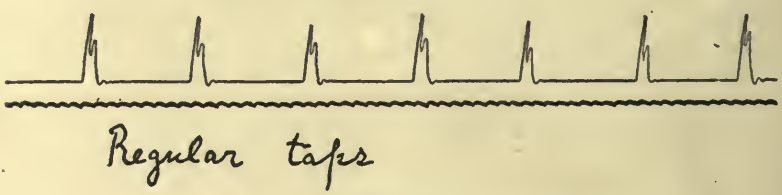


Fig. 1.

Fig. 1 shows a series of regular taps made by the finger. Here regularity is the dominant characteristic. But regularity is seen to be of two distinct sorts. Regularity of

recurrence appears in the duration of the taps and the interval between them, and over against this is regularity in the performance itself, which manifests itself in the force of the movement. In the record the duration is measured horizontally and the force vertically. The degree of regularity

Table I

Regular Taps

Showing, for each Subject, the Mean Variation, in per cent., from the average of 20 taps; with regard to the Force and Duration of the taps.

Subject	Force	Duration
1. Mr. Bates	6.22	3.42
2. Mr. Detter	9.02	6.44
3. Mr. Dignan	5.78	2.66
4. Miss Fisher	11.79	2.40
5. Mr. Folte	16.90	4.28
6. Mr. Ham	5.90	3.12
7. Miss Hendee	10.80	5.69
8. Mr. Jackson	8.68	3.81
9. Miss Noteware	6.78	3.03
10. Mr. Robinson	5.94	3.37
11. Miss Replogle	15.50	10.00
12. Mrs. Stanley	13.60	3.31
13. Miss Umphred	11.90	3.50
14. Miss Way	10.95	4.38
15. Mr. Whisman	5.92	4.99
Average	9.71	4.36

is found by computing the mean variation from the average of a number of successive movements.[1] Table I. shows the variation in force and duration of a series of twenty taps with the index finger on the rubber head of a drum connected with a recording tambour. The subjects[2] were requested to tap as regularly as possible in time and force and at a rate of their own choosing. On the whole, averaging the records of the subjects, these taps vary more than twice as much in force as in time. In other words their duration is more than twice as regular as their structural form. For some persons

[1] The measurements of the movements themselves are of no importance in the present connection, and for the sake of clearness only the variations from their average are mentioned. Throughout this paper the mean variation is expressed in per cent. of the quantity which varies.

[2] I am indebted to fifteen of my students in the University of California summer session of 1910 for the records upon which this report is based.

the variation in time is more nearly equal to that in force, but in no case is there less variation in force than in time.

It is evident that this series tends to conserve a regular succession of its elements rather than regularity in their form. But this sacrifice of form to duration occurs in a series which does not profess to have much form, and the further question arises whether the same thing would hold in a truly rhythmic series.

II

Fig. 2 shows a series of taps in rhythm. The subjects were instructed to tap in a natural manner in an iambic rhythm, and the explanation was made that this rhythm has the second beat accented or longer than the first, but no

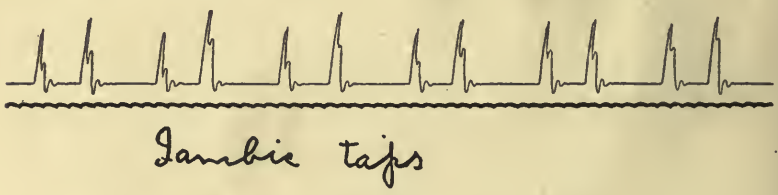


FIG. 2.

indication was given as to the relative importance of the temporal or accentual features. Table II. shows for the fifteen subjects the variation in force and duration of the first (short) and second (long) tap, based on the average of a series of ten pairs of taps. The same table shows the variation in total duration of the rhythmic element (the foot, or sum of two successive taps) and of the total force exerted in the element as measured by the sum of the forces exerted on the two parts of it. This table also gives a measure of the structural constancy of the elements in the series both in force and time. This is shown in the column headed "ratio" by the variation, in the two respects, of the ratio between the two parts of the foot when the long or accented part is divided by the short part.[1]

The more complicated performance demanded by this part of the experiment brings out more individual differences

[1] Again only the variations are considered. The actual ratios will be spoken of later on.

Table II

Iambic Taps

Showing for each Subject the Mean Variation in per cent. from the average of 10 pairs of taps, with regard to the Force and Duration of the taps. The variation is given for the first or Short tap, for the second or Long tap, for the Sum of the two taps, and for the Ratio found by dividing the short into the long tap.

Subject	Short		Long		Sum		Ratio	
	F	D	F	D	F	D	F	D
1	5.71	5.64	1.76	5.94	2.98	3.31	5.32	3.48
2	9.91	5.25	6.64	5.92	4.98	4.19	14.16	8.32
3	9.17	4.55	6.02	2.60	4.02	2.20	11.44	6.35
4	12.71	5.17	9.02	3.56	10.07	3.34	11.83	6.04
5	6.21	6.22	5.83	3.43	3.46	4.31	11.41	5.97
6	16.32	4.48	5.49	2.66	6.06	2.64	19.11	4.62
7	15.00	5.24	7.81	6.24	6.43	4.54	14.50	5.82
8	12.98	6.32	8.92	6.54	4.10	3.79	19.38	9.92
9	5.28	4.59	6.18	5.70	4.11	2.86	6.91	4.68
10	7.81	2.39	5.42	4.90	4.68	2.35	10.75	6.74
11	9.83	4.02	10.56	6.14	6.36	5.32	16.42	4.69
12	14.60	4.44	6.54	2.19	8.31	2.72	12.82	5.17
13	11.40	3.40	7.31	4.18	8.31	3.20	10.80	5.51
14	10.05	6.08	6.00	3.87	3.53	4.57	14.60	7.26
15	9.14	4.48	4.78	4.25	5.04	3.82	10.42	4.04
Average.	10.41	4.82	6.55	4.54	5.50	3.54	12.66	5.91

between the subjects, but in the long run and with very few exceptions the indications of the regular tap series are borne out by the rhythmic series. Whether we consider the short initial stroke, the long accented stroke, or their sum (the whole foot), there is greater variation in force than in time. It should be noted, however, that except for the initial stroke, there is less difference between time and force than in the case of regular taps. Taking the whole foot as a basis for comparion with the regular taps of the first series it is seen that the introduction of rhythmic form has tended to steady the movements both in time and force, but has reduced the variation in force more than in time.

The column headed 'ratio' (Table II.) shows the maintenance of internal structure in the foot or rhythmic unit. On the whole the temporal structure is maintained twice as well as the accentual structure. In neither respect is the internal structure preserved half as well as the uniformity of the rhythmic unit; the ratios vary twice as much as the feet.

III

The third part of the experiment (Fig. 3 and Table III.) reports a series of syllables spoken in iambic tetrameter rhythm and recorded by a tambour in the manner indicated in an earlier paper of the writer's.[1] The subjects were instructed to speak, in a natural rhythm, the written line:

Ta tá, ta tá, ta tá, ta tá;

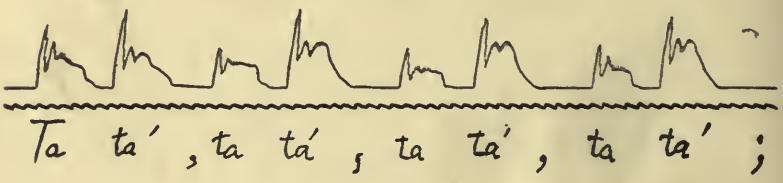


Fig. 3.

Table III

Iambic Syllables

Showing for each subject the Mean Variation in per cent. from the average of ten pairs of syllables from the first foot of the iambic tetrameter line Ta tá, ta tá, ta tá, ta tá; with regard to Force and Duration. The variation is given for the first two syllables separately, for their Sum, and for the Ratio found by dividing the first into the second.

Subject	Ta		tá		Sum		Ratio	
	F	D	F	D	F	D	F	D
1	10.62	4.14	12.56	3.82	11.18	2.76	5.94	9.05
2	9.22	4.29	8.84	5.27	6.23	3.80	9.79	5.45
3	36.00	8.85	18.38	13.80	23.40	9.22	22.00	8.32
4	40.30	10.37	20.30	7.00	22.45	6.33	43.60	11.22
5	15.92	7.34	11.48	2.96	11.82	2.44	15.60	8.58
6	24.70	4.42	30.80	8.08	23.50	3.94	27.40	12.65
7	13.80	11.68	9.14	6.26	4.67	7.10	22.10	8.13
8	13.60	7.04	12.50	5.51	11.80	4.90	12.63	5.76
9	28.00	6.55	19.50	4.77	17.00	4.06	24.70	8.52
10	16.77	7.23	15.43	12.51	15.25	8.22	20.50	15.18
11	15.30	10.90	16.40	8.27	15.50	4.39	9.68	16.65
12	13.55	5.67	26.84	7.00	17.00	5.57	22.10	5.81
13	34.90	21.40	37.20	11.24	25.30	5.39	43.70	28.30
14	17.93	11.00	16.82	6.81	14.50	5.97	22.40	8.23
15	25.80	6.63	11.53	4.13	15.43	4.41	23.50	10.76
Average.	21.09	8.50	17.81	7.16	15.67	5.23	21.71	10.84

like a line of verse, and then to repeat the same line as another verse and so on until stopped. The first foot of each of ten

[1] 'Time in English Verse Rhythm,' *Archives of Psychol.*, No. 10, 1908.

verses was measured for the record. The force is the height of the consonant, and the duration is the time from the beginning of one consonant to the beginning of the next. Here all the conditions, rhythmic form, motor performance, and experimental conditions for making the record, were more complicated than in the case of taps. The difficulty of securing an adequate record of the force of the vocal utterance is a particularly serious source of error,[1] and yet when we remember that the syllables to be measured are all made up of the same letters or sounds the data may be considered sufficiently reliable for our purposes. As compared with the iambic taps there is a very large increase in variability in all directions, and while the larger variation in force may be attributable to instrumental difficulties, the loss of control in time is beyond question. This greater variability indicates that the greater elaborateness of the rhythm or the change in the motor mechanism, or the experimental embarrassments (which were not grave) singly, or together, interfered with the rhythm in respect both to internal structure and to the relation of the units to one another.

Yet apart from greater irregularity, the spoken iambs do not differ materially from those that were tapped, when we consider that the blame for the relatively greater variability in force can properly be laid to instrumental difficulties. In Table II. the difference between time and force was more noticeable in the ratio column than in any other, but in the present case the ratios differ in force only twice as much as in duration, while in the other columns the force varies nearly three times as much as the duration. This is not to be taken as improvement in the relative regularity of structure as regards force, but again as a peculiarity of the recording device. This device might well record the relative emphasis, *i. e.*, the ratio of force, within the foot, while it would not report correctly the emphasis in different feet, or over any considerable length of time.

[1] See 'Time in Eng. Verse Rhythm,' p. 22; and compare Bourdon in *L'année psychologique*, IV., 1898, p. 370.

IV

In the fourth part of the experiment the subjects were instructed to recite into the recording apparatus in a natural but forceful manner the nursery verses:

Pease porridge hot,
Pease porridge cold,
Pease porridge in the pot nine days old.

Table IV

PEASE PORRIDGE

Showing for each Subject the Mean Variation in per cent. from an average of 10 measurements of the words Pease porridge cold; with regard to Force and Duration. The Variation is shown for each word separately, for the Sum of the three words, and for the Ratio found by dividing the first word into the second.

Subject	Pease		Porridge		Cold		Sum		Ratio	
	F	D	F	D	F	D	F	D	F	D
1	15.28	6.06	10.86	4.02	14.29	7.74	9.75	3.87	14.68	3.08
2	23.20	11.55	29.60	7.11	22.40	6.01	21.20	5.71	32.80	14.05
3	12.40	8.87	13.00	8.28	10.17	6.80	8.07	3.85	22.05	14.51
4	[1]	[1]	[1]	[1]	[1]	[1]	[1]	[1]	[1]	[1]
5	[1]	10.90	[1]	8.30	[1]	6.37	[1]	6.74	[1]	12.77
6	25.15	9.77	37.10	4.11	31.85	7.76	22.75	5.54	46.70	8.01
7	26.60	7.46	15.80	3.66	15.80	3.45	16.30	3.16	22.80	7.25
8	17.15	13.50	17.90	7.26	18.00	5.54	14.53	4.87	15.20	17.70
9	16.00	6.00	16.60	3.84	31.70	8.00	17.20	3.84	15.40	4.03
10	17.88	6.50	16.67	6.24	40.20	8.04	16.93	3.56	25.60	9.85
11	20.20	8.98	22.20	11.18	25.80	3.96	16.60	3.36	23.00	19.30
12	11.90	4.72	10.70	1.98	32.40	4.04	7.71	1.65	16.65	4.66
13	25.70	7.05	28.00	5.61	32.40	8.92	15.40	4.72	43.00	10.60
14	9.58	6.02	13.40	6.87	31.20	12.40	11.15	6.91	19.25	9.64
15	24.80	6.19	17.60	3.44	25.70	6.81	13.00	4.40	38.60	6.78
Av.	18.91	8.11	19.19	5.86	25.54	6.84	14.66	4.44	25.82	10.16

This was repeated a number of times. The record, given in Fig. 4 and Table IV., shows the variation in force (height

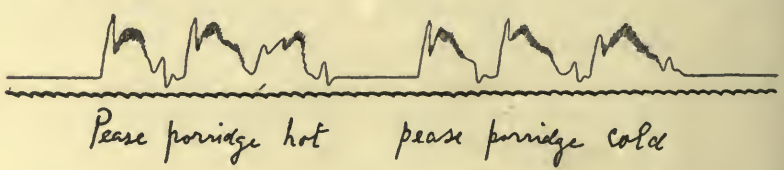


FIG. 4.

of the initial consonants p, p and c) in the phrase 'Pease porridge cold,' and in time from the beginning of pease to the

[1] No legible record.

beginning of porridge, from the beginning of porridge to the beginning of cold, and in the duration of the word cold to the end of the final consonant. It also shows the variations in time and force for the whole phrase of three words and in the ratios for the first and second words of the phrase. The variations are based on the average of ten records except that in three cases there were only eight available records. As elsewhere the variations are expressed in per cent. of the quantities involved.

With regard to either the time or force, the variations in this series are about as large for the separate parts of the foot as in the iambic syllable (ta tá) series. The variation in force exceeds the variation in time in about the same proportion as in that series. But the variations for the foot as a whole fall below those for the iambic syllables. In duration these feet are about as regular as the regular taps of the first series. The ratios present a different aspect, for while in duration they are slightly more regular than the iambic syllables, they are less regular in force. In comparison with the series of iambic syllables the 'Pease porridge' series is more regular in recurrence in regard to both the time and force of the total foot. The 'Pease porridge' series has also a more regular internal structure (ratio) in regard to time but not in regard to force. Loss of control over the relative force of the movements is very evident in even a superficial inspection of the 'Pease porridge' records. The relative force of the two p's often undergoes a complete reversal from verse to verse. The rhythm of this verse is of course irregular or even amorphic but it is of a very distinctly temporal type, giving, all the disturbing factors being considered, very great regularity in the matter of recurrence, together with a relatively high degree of regularity in internal structure so far as the time relations are concerned.

With these concrete examples from which to start we are now in a position to discuss with more understanding the temporal and accentual features in any rhythm. The first point to be observed is that all recurrence is a temporal

matter. A rhythm is temporal in so far as there is any regular return of similar features. But at the same time such a rhythm will also be accentual since there must always be points of emphasis whose return can be marked. At this stage of the discussion the question to be answered is: Which is fundamental to the rhythm; the uniform time of recurrence, or the uniform character of the thing that recurs? Assuming a reliable method of measuring both the thing and its rate of recurrence I propose the variability as a test in this question. If the movements or sounds vary in intensity more than in duration or more than the interval separating them, I submit that the rhythm is primarily temporal. No very extended argument seems to be required in support of this view, for regularity is essential to rhythm, and if the regularity is predominantly in the time relations the rhythm may be presumed to have its seat there also. Accent may be a necessary feature but it is not the distinctive feature in the rhythms examined for this study.

But we can not stop the discussion on the level of mere recurrence; that would be to stop with the whole feet, ignoring their parts. We can not consider the mere intensity and duration of the item which recurs; we must also consider its individual make-up or structural character. This puts us on the second level, that of the structure of the elements composing the rhythm. Here we find again intensities and durations, or to speak more accurately, relations of intensity and duration. May it not be that we shall find in one or the other of these relations something that will present greater regularity than the mere recurrence of the elements? If so we may look here for the essence of the rhythm.

On the surface of the returns the ratios which represent these relations appear to be more variable than the periods of recurrence. But as the writer has stated before,[1] this view although probably correct can not be taken as final, because the ratios are apparently incommensurable with the actual durations. But we can face this difficulty fairly, and still say that on this second level of structural form, consider-

[1] 'Time in English Verse Rhythm,' p. 67.

ing the ratios independently, it is in the temporal structure and not in the accentual structure that we find the greater regularity. If the fundamental regularity does reside in the arrangement of the parts within the units rather than in the recurrence of these units, it is still to be sought in the arrangement of the parts in time. In any event the predominant regularity on either level is a temporal regularity.

V

A somewhat different method of attack leads to the same conclusion. If, in Table V., we consider the actual ratios which represent the structure of the elements, we find that the ratios for force of movement are different from the ratios

Table V

Ratios

Showing for each Subject the average absolute amount of the Ratio found by dividing the first tap or syllable into the second; with regard to Force and Duration.

Subject.	Iambic Taps Cf. Table II		Ta tá Cf. Table III		Pease porridge Cf. Table IV	
	F	D	F	D	F	D
1	1.22	1.78	1.68	2.18	1.09	1.10
2	1.29	2.45	1.23	1.96	.86	1.14
3	1.38	1.71	1.89	2.56	1.14	1.33
4	1.57	2.52	2.46	2.56	——	——
5	1.28	1.73	2.24	2.93	——	1.14
6	1.87	.95	1.55	1.71	1.02	1.14
7	1.87	1.61	1.92	2.64	.98	1.09
8	1.58	1.19	.94	1.91	.98	1.12
9	1.09	1.79	2.44	3.06	.99	1.02
10	1.24	1.07	1.53	2.47	.55	.94
11	1.62	2.94	1.05	2.13	.93	1.16
12	1.27	2.10	1.14	2.00	.72	1.05
13	1.64	1.82	2.82	2.84	1.16	1.08
14	1.17	1.96	1.77	2.22	.67	1.13
15	1.52	2.47	2.11	3.31	.86	1.49
Average.	1.44	1.87	1.78	2.43	.92	1.14

for duration of movement. In both of the iambic series the time-ratios are larger than the force-ratios. That is to say, the typical iambic structure with the second part outweighing the first part is more adequately or fully carried out in time than in stress. If the essence of the rhythm is on the second level, in the arrangement of the parts within the rhythmic

unit, then again the arrangement in time satisfies the requirements more fully.

No inferences can be drawn from the absolute ratios of the 'Pease porridge' records because there is no evidence that this was meant by the speakers to be an iambic ryhthm. It has some of the ear-marks of a trochaic rhythm[1] and in that case we do not know whether a larger or smaller ratio stands for the typical rhythm. In fact the absolute ratio fails when applied to verse rhythms because of the fact here illustrated that the time-ratio is determined in such cases by the necessary time of uttering the words. The second word in this verse appears to be the second element in a trochaic foot; it sounds so to the ear and its initial consonant shows in the record less stress, on the average, than the initial consonant of the first word; and yet it takes longer to say the longer and more complex second word. In verse there are no typical time-ratios and the force-ratios are not easily measured. With this exception the evidence from the absolute ratio furnishes a valuable check upon the evidence from the relative variabilities.

These results are presented with the hope of arousing interest in what appears to me to be a promising method for the analysis of some of the fundamental questions regarding the basis of rhythmic actions and impressions. The results themselves, so far as they go, indicate as I think, that the time aspects are fundamental and that the accentual features while necessary are not at the root of the phenomena.

[1] *Op. cit.*, p. 55.

XVII. Some Preferences by Boys and Girls as Shown in their Choice of Words

BY M. I. STOCKTON

The broader purpose of the experiments reported in this paper was to obtain light upon possible differences in the affective life of children at different ages, and especially upon any differences between boys and girls. The present experiments were limited, however, to a study of the difference between boys and girls as shown in their choice between time and space words; between words expressing activity and those expressing passivity; between words relating to dress and those relating to food; between verbs and adjectives.

These experiments were suggested by the conclusions stated in Mrs. Manchester's paper on 'Unreflective Ideas of Men and Women.'[1] The question in her study was whether there is any difference between college men and women in their unreflective ideas. From the results of her experiment she drew the following general conclusions:

1. The surface ideas of men and women pertain to objects which are familiar and interesting.

2. The dynamic aspect of objects is more attractive to men, while the static or completed aspect appeals more to women.

3. Time as a factor enters more largely into the surface ideas of men; space is more often a prominent feature of the surface ideas of women.

4. Men make a greater use of abstract terms, while women show a preference for concrete and descriptive words.

The present experiments were carried out in all the grades, above the low second, of the Bay Grammar School, Oakland, Cal. The accompanying table (Table A) gives the age, number and distribution of the pupils participating.

[1] Psychological Review, Vol. 12 (1905), p. 50.

Method.—It was thought that if a pair of words of equal difficulty and familiarity were offered, one of which was to be selected, although several motives might affect the choice, yet perhaps there might, after elimination and check, be discovered some clue to the attractiveness of the ideas themselves. Four lists of twenty pairs of words were selected. In the first list, each pair was composed of a time word and a space word; in the second list, of a word denoting activity and one denoting passivity; in the third list, of one referring to dress and one referring to food; in the fourth list, of a verb and an adjective.

TABLE A

Grade	Average Number of Pupils Participating in the Four Sets of Experiments		Average Age	
	Boys	Girls	Boys	Girls
B2	21	19	9.3	8.6
A3	20	17	10.1	9.3
B3	14	17	10.9	9.3
A4	21	17	10.9	11
B4	9	8	12.4	11.7
A5	6	17	11.3	12.1
B5	11	15	13.9	12.1
A6	18	12	13.3	12.9
B6	4	8	13.6	14
A7	14	13	13.6	12.9
B7	6	9	14.5	14
A8	4	6	14.6	14.3
B8	2	4	15.6	15.3

Each pair of words was written, one directly under the other, on a large card which every pupil could see distinctly when the teacher showed it to the class. The cards were numbered so that the words could be presented in order. The words were so written that if a time word was first on one card, a space word was first on the next following; a time word on the third and so on. In this way, any preference due to the position of the word on the card would be offset. Such an order was carefully followed in each of the four lists, as will be seen by glancing at the words on pages 349 and 350.

Before the cards were presented, paper was distributed to the pupils. They were told to write only one of the two words that appeared on each card. No further information concerning the work was given. With the exception of the

third set of papers from the eighth grade, the words were written by the pupils of the twelve different classes at the the same hour on each of the four days.

The lists containing time and space words and words denoting activity and passivity were given on the first day. These have been designated Set I. Set II. was given five weeks later and contained the list of words pertaining to dress and food and the list of verbs and adjectives. Set III. was composed of the words of Set I, but the order of the words of each pair was reversed. In this way, a time word that had been first of a pair in Set I., was second of the same pair in Set III. By this double check—that is by alternating within the set itself the class of word that appeared first on each card of the twenty, and by reversing in Set III. the position of each word in its pair as it appeared in Set I. — any influence which might be due to preference for a word merely because it was first or second in its pair would probably in the long run

List 1		List 2	
Time and Space Words		**Words Denoting Activity and Passivity**	
mile	little	run	romp
year	month	sit	hush
always	soon	rest	calm
under	tall	busy	move
big	down	swim	lively
now	daily	still	softly
later	tomorrow	sleep	silent
small	thickness	work	hurry
where	width	talking	chatter
when	quick	listen	slumber
hour	seldom	quiet	patient
high	narrow	throw	speaking
inch	above	dig	climb
slow	fast	wait	float
early	yesterday	idle	grow
large	outside	jump	push
below	broad	play	day
today	until	ride	night
then	time	evening	standing
there	space	morning	chasing

List 3 Words Relating to Dress and Food		List 4 Verbs and Adjectives	
dress	collar	build	sing
grapes	cheese	little	poor
nut	banana	fresh	old
cap	ribbon	threw	buy
hat	velvet	give	sailed
pie	apple	hot	happy
berries	supper	large	green
gloves	cloak	grow	lived
coat	silk	tell	sold
cake	food	good	glad
bread	beef	rich	long
skirt	lace	pick	make
vest	tailor	looked	run
meat	turkey	short	red
fruit	pudding	sweet	kind
shoes	necktie	think	jump
stockings	button	follow	slept
potatoes	carrot	sunny	small
butter	dinner	fine	merry
woolen	shawl	eat	break

be compensated. The third set was presented two weeks after the second set. Set IV. contained the words of Set II. in reverse order and was given three weeks later than Set III.

The pupils of the A second grade because of insufficient power of writing were unsuited to the experiment; so the B second grade is the youngest class employed, and is referred to simply as the second grade. There were so few pupils in the eighth grade that the results of the two divisions together have been given as the eighth grade. The work of the A fourth grade was interrupted on two mornings, so there is only one set of papers for the list of verbs and adjectives and for the list containing words denoting activity and passivity.

Many difficulties beset one in preparing the lists of words. There were serious limitations placed upon selection by the difference in ability of the various classes. It was necessary that the two words of a pair, fairly intelligible and within the vocabulary of the pupils, should be of the same length

and of the same degree of difficulty in writing. Although much time was spent in preparing the lists, it is impossible to feel that they are entirely satisfactory or beyond criticism.

The results are arranged in Tables I. to IV. which show the percentage of each class of words selected by the boys and girls respectively in the different grades and also the percentage of first and second words selected in the same lists. The percentages are given for each set of papers obtained from each of the four lists of words and also the average percentage of the two sets.

In Table V., the total number of each class of words selected by the twelve grades is given with the corresponding number of first and second words chosen. The percentage selected is also shown. The more significant figures are those given as percentages of the different classes of words selected. The total number of words is of less value since the proportion of boys and of girls varies in the different grades.

Plates A to D inclusive give in different form the same results as are given in Tables I. to IV. The ordinates represent the percentages of preference; the abscissæ represent in order the different school grades. The average value of each curve for all twelve grades is also shown in each table by the horizontal lines; *e. g.*, the horizontal line composed of a dot and a dash in Plate A represents the average selection of time words for the twelve grades for the boys,—having a value of 53.9 per cent. Plate E shows the average percentage of the different classes of words and of first and second words for the twelve grades combined.

Let us consider, first, what conclusions one is warranted in drawing with regard to the more limited problem of this experiment; and later the relation of these results to the larger field will be of interest.

The averages of the two sets in Tables I.–IV. are the significant figures; for any apparent preference due merely to the order of the words is probably eliminated by the alternation and reversal of words explained on page 348. From Table I. and its graphic presentation in Plates A and E, one

Table I

Percentage of Words Chosen

Character of the Word	Boys				Girls			
	Time	Space	First	Second	Time	Space	First	Second
Second Grade.								
First set	54.2	45.8	43.9	56.1	48.6	51.4	50.5	49.5
Third set	49.6	50.4	42.9	57.1	50.4	49.6	62.1	37.9
Average	51.8	48.2	43.4	56.6	49.5	50.5	56.3	43.7
A Third Grade.								
First set	53.7	46.3	56.3	43.7	53.1	46.9	62.7	37.3
Third set	57.4	42.6	60.4	39.6	53.3	46.7	64.2	35.8
Average	55.5	44.5	58.4	41.6	53.2	46.8	63.4	36.6
B Third Grade.								
First set	55.4	44.6	37.6	62.4	52.5	47.5	37.2	62.8
Third set	51.0	49.0	69.2	30.8	51.0	49.0	69.2	30.8
Average	53.2	46.8	53.4	46.6	51.7	48.3	53.2	46.8
A Fourth Grade.								
First set	56.2	43.8	50.7	49.3	53.0	47.0	51.0	49.0
Third set	54.6	45.4	58.9	41.1	43.3	56.7	53.9	46.1
Average	55.4	44.6	54.8	45.2	48.1	51.9	52.4	47.6
B Fourth Grade.								
First set	50.0	50.0	55.5	44.5	51.9	48.1	33.1	66.9
Third set	50.2	49.8	72.3	27.7	53.4	46.6	46.6	53.4
Average	50.1	49.9	63.9	36.1	52.6	47.4	39.8	60.2
A Fifth Grade.								
First set	54.2	45.8	34.2	65.8	50.9	49.1	42.9	57.1
Third set	50.7	49.3	62.1	37.9	52.8	47.2	47.2	42.8
Average	52.5	47.5	48.2	51.8	51.8	48.2	50.1	49.9
B Fifth Grade.								
First set	49.0	51.0	40.0	60.0	55.7	44.3	44.3	55.7
Third set	56.0	44.0	61.4	38.6	55.4	44.6	59.3	40.7
Average	52.5	47.5	50.7	49.3	55.5	44.5	51.8	48.2
A Sixth Grade.								
First set	54.7	45.3	52.2	47.8	48.8	51.2	51.2	48.8
Third set	51.4	48.6	63.9	36.1	53.9	46.1	70.7	29.3
Average	53.0	47.0	58.1	41.9	51.3	48.7	60.9	39.1
B Sixth Grade.								
First set	48.8	51.2	53.7	46.3	50.7	49.3	67.8	32.2
Third set	55.0	45.0	55.0	45.0	50.0	50.0	74.3	25.7
Average	51.9	48.1	54.4	45.6	50.3	49.7	71.1	28.9
A Seventh Grade.								
First set	56.6	43.4	44.6	55.4	53.7	46.3	48.9	51.1
Third set	58.8	41.2	64.3	35.7	55.9	44.1	67.2	32.8
Average	57.7	42.3	54.5	45.5	54.8	45.2	58.1	41.9
B Seventh Grade.								
First set	58.8	41.2	43.8	56.2	55.7	44.3	52.2	47.8
Third set	47.9	52.1	68.9	31.1	69.5	30.5	58.7	41.2
Average	53.3	46.7	56.4	43.6	62.6	37.4	55.5	44.5
Eighth Grade.								
First set	60.8	39.2	53.3	46.7	57.9	42.1	36.7	63.3
Third set	51.3	48.7	68.8	31.2	49.1	50.9	80.0	20.0
Average	56.0	44.0	61.1	38.9	53.5	46.5	58.3	41.7

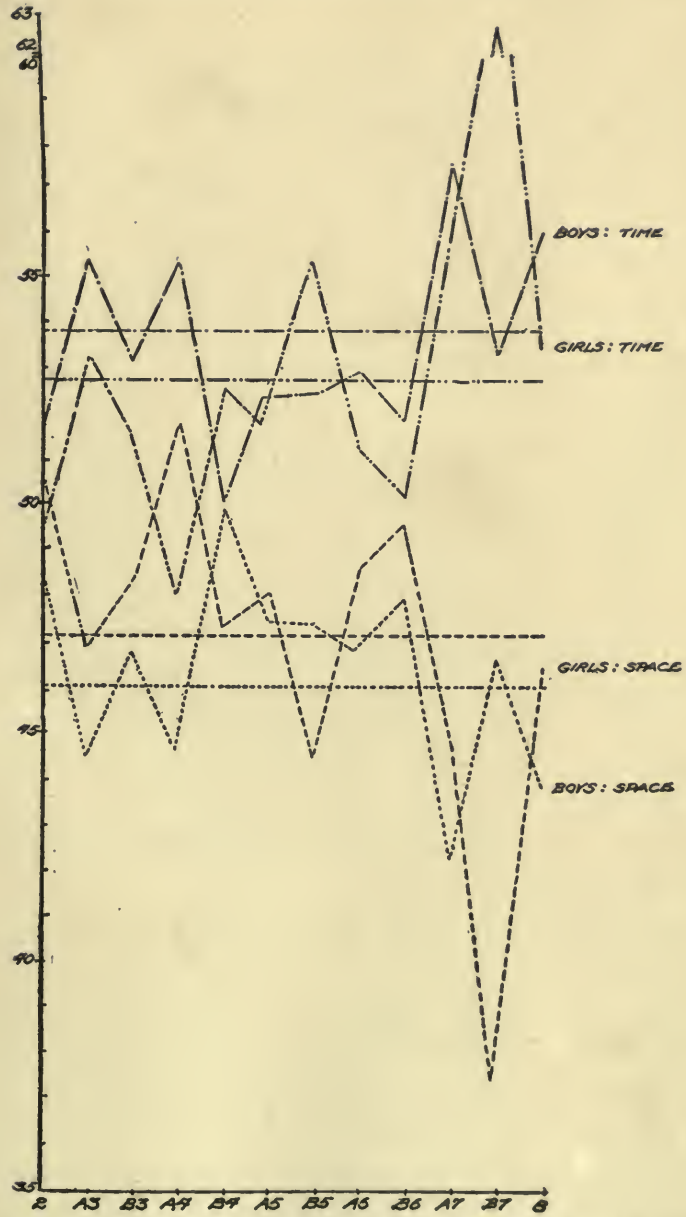


PLATE A. Choice between time-words and space-words, selected by boys and girls respectively. The ordinates indicate in per cent. the amount of preference shown for each of the two groups of words; the abscissæ, the school-grades. (From Table I.)

Table II

Percentage of Words Chosen

Character of the Word	Boys				Girls			
	Activity	Passivity	First	Second	Activity	Passivity	First	Second
Second Grade.								
First set	49.5	50.5	45.4	54.6	53.5	46.5	48.5	51.5
Third set	46.8	53.2	41.6	58.4	53.2	46.8	54.8	45.2
Average	48.2	51.8	43.5	56.5	53.4	46.6	51.6	48.4
A Third Grade.								
First set	51.6	48.4	60.5	39.5	51.1	48.9	57.1	42.9
Third set	51.8	48.2	51.5	48.5	50.0	50.0	63.4	36.6
Average	51.7	48.3	56.0	44.0	50.5	49.5	60.3	39.7
B Third Grade.								
First set	54.5	45.5	44.7	55.3	54.2	45.8	45.1	54.9
Third set	41.6	58.4	63.9	36.1	49.1	50.9	58.3	41.7
Average	48.0	52.0	54.3	45.7	51.6	48.4	51.7	48.3
A Fourth Grade.								
First set	50.9	49.1	52.9	47.1	46.0	54.0	51.0	49.0
B Fourth Grade.								
First set	48.3	51.7	60.6	39.4	43.1	56.9	39.4	60.6
Third set	51.3	48.7	67.4	32.6	36.1	63.9	57.9	42.1
Average	49.8	50.2	64.0	36.0	39.6	60.4	48.6	51.4
A Fifth Grade.								
First set	45.4	54.6	36.2	63.8	50.1	49.9	51.2	48.8
Third set	53.9	46.1	61.8	38.2	48.5	51.5	51.5	48.5
Average	49.6	50.4	49.0	51.0	49.3	50.7	51.4	48.6
B Fifth Grade.								
First set	49.5	50.5	46.5	53.5	48.1	51.9	47.5	52.5
Third set	58.9	41.1	50.3	49.7	49.2	50.8	53.1	46.9
Average	54.2	45.8	48.4	51.6	48.6	51.4	50.3	49.7
A Sixth Grade.								
First set	54.5	45.5	47.5	52.5	51.2	48.8	53.5	46.5
Third set	50.6	49.4	70.1	29.9	48.8	51.2	42.2	57.8
Average	52.5	47.5	58.8	41.2	50.0	50.0	47.8	52.2
B Sixth Grade.								
First set	56.2	43.8	71.2	28.8	47.1	52.9	72.8	27.2
Third set	54.5	45.5	48.5	51.5	43.6	56.4	73.6	26.4
Average	55.4	44.6	59.8	40.2	45.4	54.6	73.2	26.8
A Seventh Grade								
First set	51.7	48.3	52.3	47.7	50.4	49.6	56.1	43.9
Third set	57.2	42.8	59.7	40.3	45.6	54.4	56.9	43.1
Average	54.5	45.5	56.0	44.0	48.0	52.0	56.5	43.5
B Seventh Grade.								
First set	47.5	52.5	52.5	47.5	44.3	55.7	52.8	47.2
Third set	49.1	50.9	70.2	29.8	43.5	56.5	57.1	42.9
Average	48.3	51.7	61.4	38.6	43.9	56.1	54.9	45.1
Eighth Grade.								
First set	52.1	47.9	49.6	50.4	48.3	51.7	39.2	60.8
Third set	46.4	53.6	73.7	26.3	52.2	47.8	84.1	15.9
Average	49.3	50.7	61.6	38.4	50.3	49.7	61.6	38.4

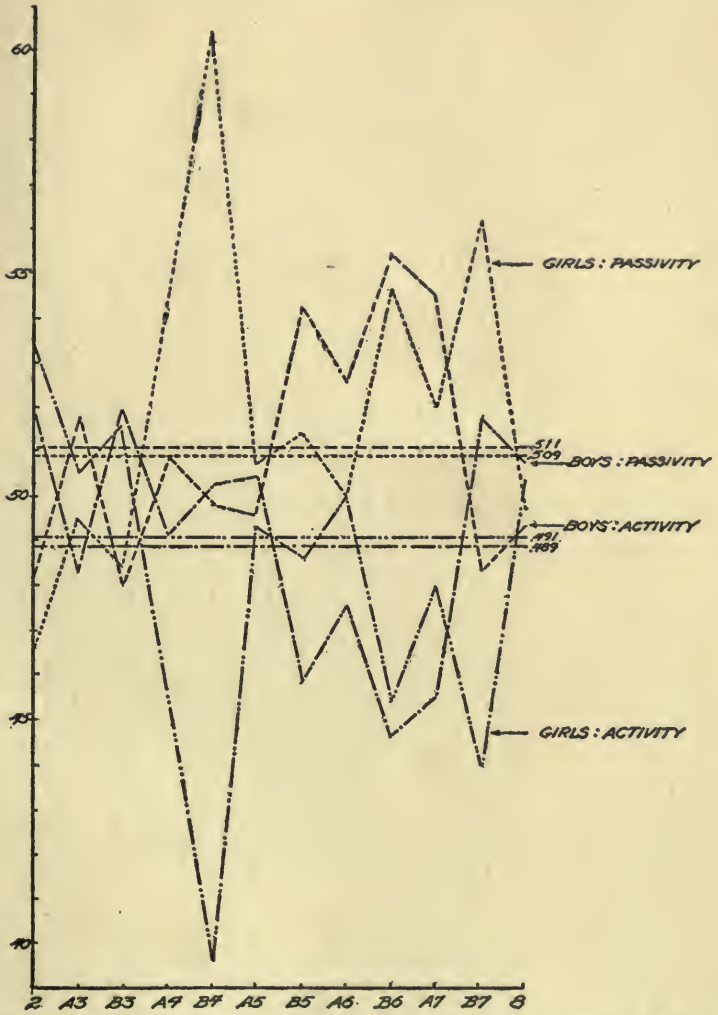


PLATE B. Choice between words denoting activity and passivity by boys and girls respectively. The ordinates indicate in per cent. the preference shown for each of the two groups of words, the abscissæ, the school-grades. (From Table II.)

may conclude that a noticeable predominance of time interest is shown by both boys and girls. There is a slightly greater predominance of time interest among the boys than among the girls. In these same sets of time and space words, there is a predominance of first words in ten grades among the boys and in eleven grades among the girls.

Table III

Percentage of Words Chosen

Character of Word Chosen	Boys				Girls			
	Dress	Food	First	Second	Dress	Food	First	Second
Second Grade.								
Second set	50.7	49.3	60.3	39.7	50.8	49.2	56.1	43.9
Fourth set	48.5	51.5	39.6	60.4	37.3	62.7	51.7	48.3
Average	49.6	50.4	49.9	50.1	44.1	55.9	53.9	46.1
A Third Grade.								
Second set	49.9	50.1	41.3	58.7	58.1	41.9	52.7	47.3
Fourth set	49.7	50.3	72.6	27.4	50.8	49.2	72.1	27.9
Average	49.8	50.2	56.9	43.1	54.5	45.5	62.4	37.6
B Third Grade.								
Second set	41.1	58.9	53.3	46.7	41.1	58.9	53.5	46.5
Fourth set	55.4	44.6	64.8	35.2	44.5	55.5	57.4	42.6
Average	48.3	51.7	59.1	40.9	42.8	57.2	55.5	44.5
A Fourth Grade.								
Second set	40.0	60.0	59.0	41.0	39.6	60.4	46.4	53.6
Fourth set	42.9	57.1	57.1	42.9	43.1	56.9	57.3	42.7
Average	41.5	58.5	58.1	41.9	41.4	58.6	51.8	48.2
B Fourth Grade.								
Second set	49.7	50.3	61.3	38.7	45.0	55.0	42.9	57.1
Fourth set	45.5	54.5	57.1	42.9	47.6	52.4	59.5	40.5
Average	47.6	52.4	59.2	40.8	46.3	53.7	51.2	48.8
A Fifth Grade.								
Second set	42.2	57.8	51.9	48.1	49.1	50.9	52.3	47.7
Fourth set	41.7	58.3	44.8	55.2	49.4	50.6	57.9	42.1
Average	41.9	58.1	48.4	51.6	49.3	50.7	55.1	44.9
B Fifth Grade.								
Second set	56.4	43.6	66.1	33.9	44.8	55.2	51.3	48.7
Fourth set	31.5	68.5	53.2	46.8	44.1	55.9	58.1	41.9
Average	43.9	56.1	59.6	40.4	44.5	55.5	54.7	45.3
A Sixth Grade.								
Second set	38.5	61.5	63.6	36.4	55.9	44.1	67.9	32.1
Fourth set	28.9	71.1	66.1	33.9	52.9	47.1	67.1	32.9
Average	33.7	66.3	64.8	35.2	54.4	45.6	67.5	32.5
B Sixth Grade.								
Second set	30.0	70.0	57.5	42.5	44.1	55.9	77.4	22.6
Fourth set	20.0	80.0	65.0	35.0	45.0	55.0	67.5	32.5
Average	25.0	75.0	61.3	38.7	44.5	55.5	72.5	27.5
A Seventh Grade.								
Second set	37.6	62.4	56.2	43.8	37.7	62.3	45.8	54.2
Fourth set	18.6	81.4	53.6	46.4	24.1	75.9	55.5	44.5
Average	28.1	71.9	54.9	45.1	30.9	69.1	50.6	49.4
B Seventh Grade.								
Second set	52.5	47.5	50.8	49.2	53.6	46.4	50.3	49.7
Fourth set	34.2	65.8	42.5	57.5	40.6	59.4	57.3	42.7
Average	43.4	56.6	46.6	53.4	47.1	52.9	53.8	46.2
Eighth Grade.								
Second set	45.8	54.2	57.5	42.5	47.1	52.9	53.8	46.2
Fourth set	38.8	61.2	71.2	28.8	45.9	54.1	58.7	41.3
Average	42.3	57.7	64.4	35.6	46.5	53.5	56.3	43.7

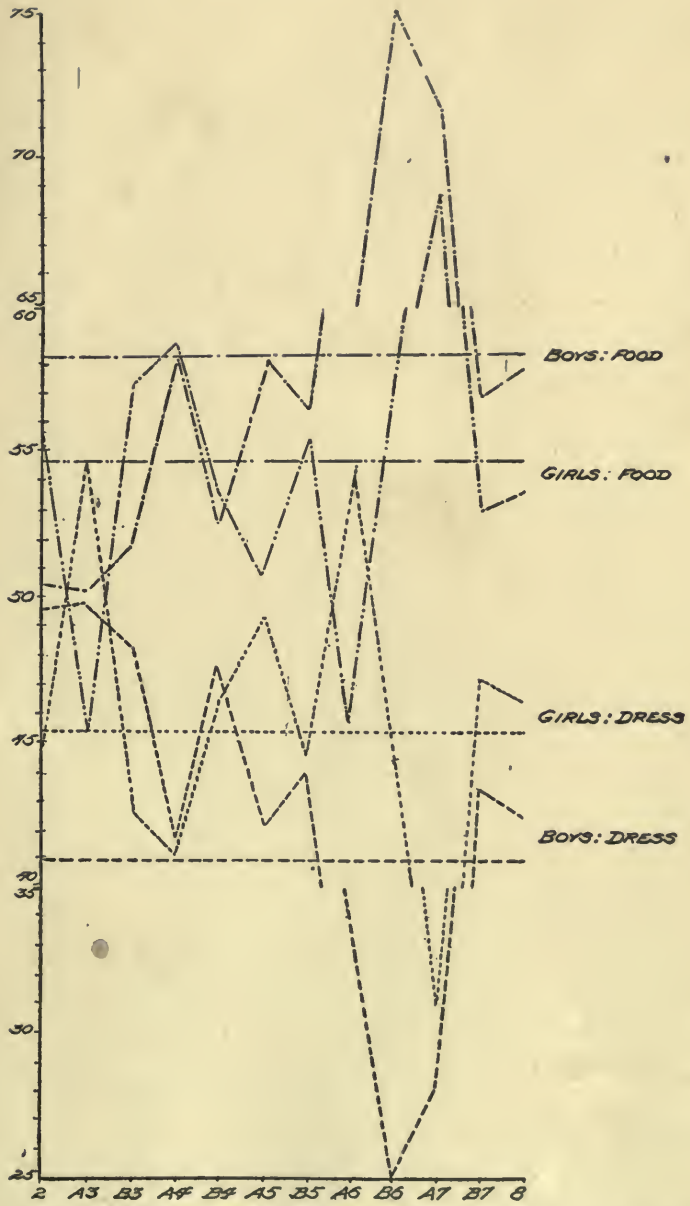


PLATE C. Choice between words denoting food and dress, by boys and girls respectively. The ordinates indicate in per cent. the amount of preference shown for each of the two groups of words; the abscissæ, the school-grades. (From Table III.)

Table IV

Percentage of Words Chosen

Character of Word Chosen	Boys				Girls			
	Verbs	Adjectives	First	Second	Verbs	Adjectives	First	Second
Second Grade.								
Second set	45.8	54.2	58.7	41.3	46.8	53.2	59.5	40.5
Fourth set	45.3	54.7	40.8	59.3	36.4	63.6	54.4	45.6
Average	45.5	54.5	49.7	50.3	41.6	58.4	56.9	43.1
A Third Grade.								
Second set	43.1	56.9	55.4	44.6	43.4	56.6	62.4	37.6
Fourth set	45.6	54.4	69.7	30.3	46.6	53.4	80.1	19.9
Average	44.4	55.6	62.5	37.5	45.0	55.0	71.3	28.7
B Third Grade.								
Second set	43.7	56.3	66.9	33.1	42.2	57.8	56.9	43.1
Fourth set	46.2	53.8	69.6	30.4	42.1	57.9	64.1	35.9
Average	44.9	55.1	68.3	31.7	42.2	57.8	60.5	39.5
A Fourth Grade.								
Fourth set	44.7	55.3	51.9	48.1	43.3	56.7	56.3	43.7
B Fourth Grade.								
Second set	44.0	56.0	61.0	39.0	42.5	57.5	49.1	50.9
Fourth set	43.8	56.2	67.2	32.8	38.5	61.5	63.2	36.8
Average	43.9	56.1	64.1	35.9	40.5	59.5	56.2	43.8
A Fifth Grade.								
Second set	37.2	62.8	50.0	50.0	41.2	58.8	60.8	39.2
Fourth set	47.3	52.7	48.4	51.6	43.3	56.7	62.4	37.6
Average	42.3	57.7	49.2	50.8	42.3	57.7	61.6	38.4
B Fifth Grade.								
Second set	38.8	61.2	62.7	37.3	40.7	59.3	57.7	42.3
Fourth set	42.7	57.3	50.4	49.6	36.5	63.5	55.6	44.4
Average	40.7	59.3	56.5	43.5	38.6	61.4	56.6	43.4
A Sixth Grade.								
Second set	46.9	53.1	65.2	34.8	43.3	56.7	58.9	41.1
Fourth set	42.9	57.1	69.6	30.4	51.7	48.3	57.5	42.5
Average	44.9	55.1	67.4	32.6	47.5	52.5	58.2	41.8
B Sixth Grade.								
Second set	41.9	58.1	67.6	32.4	45.6	54.4	79.4	20.6
Fourth set	40.5	59.5	50.6	49.4	41.9	58.1	70.6	29.4
Average	41.2	58.8	59.1	40.9	43.7	56.3	75.0	25.0
A Seventh Grade.								
Second set	41.9	58.1	61.1	38.9	40.7	59.3	45.6	54.4
Fourth set	39.8	60.2	59.8	40.2	38.9	61.1	54.5	45.5
Average	40.8	59.2	60.5	39.5	39.8	60.2	50.1	49.9
B Seventh Grade.								
Second set	45.8	54.2	67.5	32.5	48.1	51.9	55.8	44.2
Fourth set	49.2	50.8	44.2	55.8	41.1	58.9	58.9	41.1
Average	47.5	52.5	55.8	44.2	44.6	55.4	57.4	42.6
Eighth Grade.								
Second set	35.9	64.1	57.5	42.5	41.2	58.8	54.3	45.7
Fourth set	43.1	56.9	70.9	29.1	43.9	56.1	56.7	43.3
Average	39.5	60.5	64.2	35.8	42.5	57.5	55.5	44.5

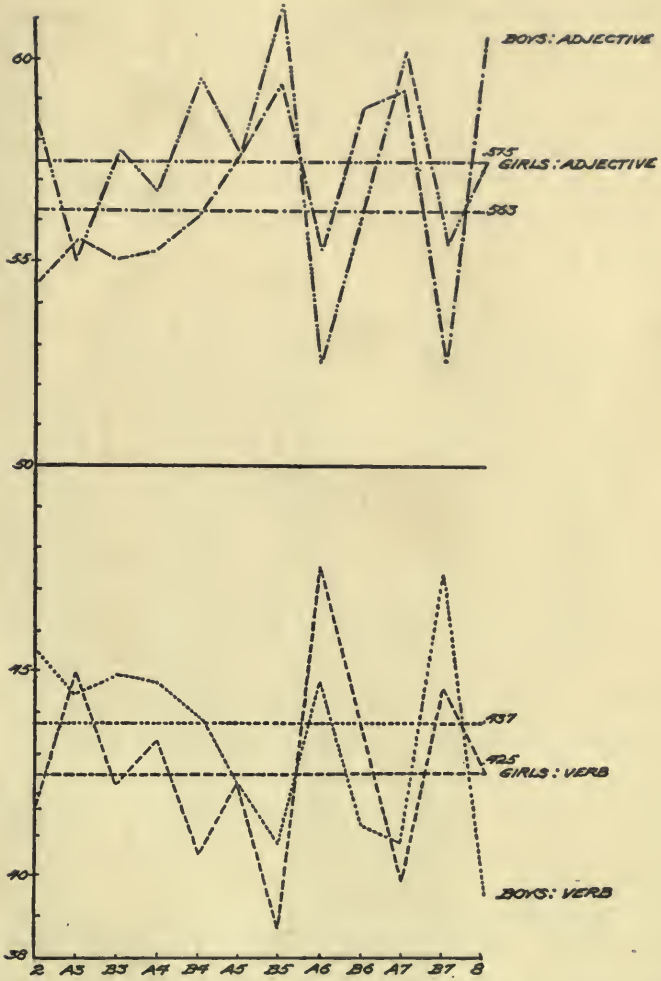


PLATE D. Choice between verbs and adjectives by boys and girls respectively. The ordinates indicate in per cent. the amount of preference shown for each of the two groups of words; the abscissæ, the school-grades. (From Table IV.)

Table II. and Plates B and E show a slight preference among the boys for words denoting activity and not quite so great a preference among the girls for words denoting passivity. The preference for first words is shown in nine grades among the boys and in ten grades among the girls.

Table III. and Plates C and E show a preference for words

pertaining to food among both boys and girls. The preference is more marked among the boys and is shown for all the grades (Plate C); whereas it is shown in ten of the grades among the girls. The greatest difference in the choice of contrasting words is shown here in the result for the boys of the B6 and A7 grades. There is a predominance here of first words in nine grades among the boys and in all grades among the girls.

TABLE V

Showing the total number and percentage of each class of words chosen and also the total number and percentage of first and second words chosen.

	Time	Space	First	Second	Activ.	Passiv.	First	Second
Boys	3206	2743	3233	2716	2812	2688	2976	2524
	53.9	46.1	54.3	45.7	51.1	48.9	54.1	45.9
Girls	3458	3084	3562	2980	3039	3153	3310	2882
	52.8	47.2	54.5	45.5	49.1	50.9	53.5	46.5
	Dress	**Food**	**First**	**Second**	**Verb**	**Adj.**	**First**	**Second**
Boys	2467	3409	3336	2540	2408	3100	3267	2241
	41.9	58.1	56.7	43.3	43.7	56.3	59.3	40.7
Girls	2942	3544	3666	2820	2584	3492	3614	2462
	45.4	54.6	56.6	43.4	42.5	57.5	59.5	40.5

Table IV. and Plates D and E show a predominance of adjectives in all the grades among both boys and girls; here the preference is greater among the girls. There is a predominance of first words among the girls in all grades and among the boys in ten grades.

Although Mrs. Manchester's work was upon men and women, nevertheless a comparison of these results with hers is interesting. The element of time was more noticeable among the ideas of men and that of space among the ideas of women. These boys and girls alike show a preference for time ideas—the preference being slightly greater among the boys. The idea of activity was characteristic of the men's lists while that of inactivity appeared in the women's lists. The boys show, on the average, a preference for words denoting activity, although even among them there is in six of the twelve grades a slight predominance of words denoting passivity. Among the girls there is a noticeable preference for words denoting passivity. The men were slightly in advance

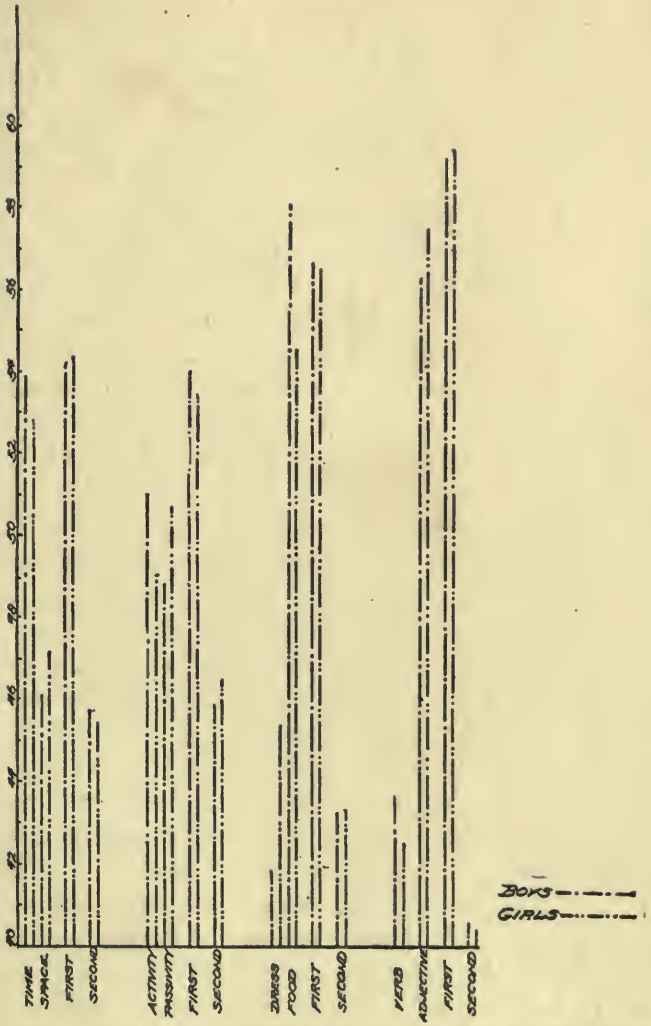


PLATE E. Choice between the various groups of words, and the choice between first and second words when the several groups were offered. (From Table V.)

of the women with reference to food; whereas the women exceeded the men in the class referring to wearing apparel. In this experiment, both boys and girls show a marked preference for words denoting food; the preference is stronger among the boys. The men led in the number of verbs written and the women in the number of adjectives. Both boys and girls show a great preference for adjectives.

The larger problem concerning the difference at various ages in the affective life of boys and girls may be considered in two aspects: (1) What may be inferred from the results of this experiment concerning the problem? (2) How are such inferences related to the conclusions of other investigators?

Considering now the first of these, Plates A to D suggest a tendency toward an increase of preference with age. That this tendency might be more carefully studied, Table VI. was prepared. This table shows in percentages the change of preference based on the different classes of ideas, with age, on the part of the boys and the girls. Table VII. shows the change of preference based on the position of the words, with age, on the part of the boys and the girls. Plate F gives in graphic form the data of Table VI.; and Plate G, the data of Table VII.

Comparing the two plates (F and G), one sees that the preference based upon the position of the words is more pronounced than the preference based upon ideas. This is certainly contrary to the expectations of the writer at the beginning of the experiments. Although one is not surprised to find the pupils in the second and third grades showing a preference for words merely according to their position, one does not look for such purely superficial preference in the upper grades. One's general observation that children grow more thoughtful after nine or ten years of age is upheld by such studies as Mrs. Mary Sheldon Barnes' and Miss Vostrovsky's. Mrs. Barnes[1] has shown that the ability of both boys and girls to make legitimate and critical inferences from an historical incident increases after eight years of age. The increase at first is gradual, then more rapid. Miss Vostrovsky[2] found an increase with age in definite answers to the question, "Why did you select your last book?" She also found an increase of disbelief in superstitions with age.[3]

Comparing Plates F and G more closely, one sees that among the girls there are five grades in which the curve for preference based upon ideas rises above 10 per cent.; in four

[1] 'Studies in Historical Method,' Boston, 1896, p. 68.

[2] 'Study of Children's Superstitions,' Barnes' 'Studies in Education,' Vol. 1, p. 123.

[3] 'Children's Reading Tastes,' *Pedagogical Seminary*, Vol. 6, p. 523.

SHOWING THE CHANGE OF PREFERENCE, IN PER CENT., WITH GRADE[1]

List of Time and Space Words

(With two exceptions, where a preference for Space is indicated by Sp., the preference is for Time words)

Grade	2	A3	B3	A4	B4	A5	B5	A6	B6	A7	B7	8
Boys	3.6[1]	11.	6.4	10.8	.2	5.	5.	6.	3.8	15.4	6.6	12.
Girls	1. (Sp.)	6.4	3.4	3.8(Sp.)	5.2	3.6	11.	2.6	.6	9.6	25.2	7.
Boys and girls combined	2.3	8.7	4.9	7.3	2.7	4.3	8.	4.3	2.2	12.5	15.9	9.5

List of Words Denoting Acitvity and Passivity

(Preference is for Activity except where Passivity is indicated by P.)

Boys	3.6(P)	3.4	4. (P)	1.8	.4(P)	.8(P)	8.4	5.	10.8	9.	3.4(P)	1.4(P)
Girls	6.8	1.	3.2	8. (P)	20.8(P)	1.4(P)	2.8(P)	0.	9.2(P)	4. (P)	12.2(P)	.6
Boys and girls combined	5.2	2.2	3.6	4.9	10.6	1.1	5.6	2.5	10.	6.5	7.8	1.

List of Words Denoting Dress and Food

(Preference is for Food except where Dress is indicated by D.)

Boys	.8	.4	3.4	17.	4.8	16.2	12.2	32.6	50.	43.8	13.2	15.4
Girls	11.8	9. (D)	14.4	17.2	7.4	1.4	11.	8.8(D)	11.	38.2	5.8	7.
Boys and girls combined	6.3	4.7	8.9	17.1	6.1	8.8	11.6	20.7	30.5	41.	9.5	11.2

List of Verbs and Adjectives

(The preferences throughout are for Adjectives)

Boys	9.	11.2	10.2	10.6	12.2	15.4	18.6	10.2	17.6	18.4	5.	21.
Girls	16.8	10.	15.6	13.4	19.5	15.4	22.8	5.	12.6	20.4	10.8	15.
Boys and girls combined	12.9	10.6	12.9	12.	15.8	15.4	20.7	7.6	15.1	19.4	7.9	18.

Average Course of Preference; all Lists Combined

Boys	4.3	6.5	6.	10.1	4.4	9.4	11.1	13.5	20.6	21.7	7.1	12.5
Girls	9.1	6.6	9.2	10.6	13.3	5.5	11.9	4.1	8.4	13.1	13.5	7.4
Boys and girls combined	6.7	6.6	7.6	10.3	8.8	7.4	11.5	8.8	14.5	17.4	10.3	9.9

[1] Table I. shows that for the boys in the second grade the average selection of time words was 51.8 per cent. of the total number of time and space words presented; the average selection of space words was 48.2 per cent. This indicates a preference of 3.6 per cent. in favor of time words.

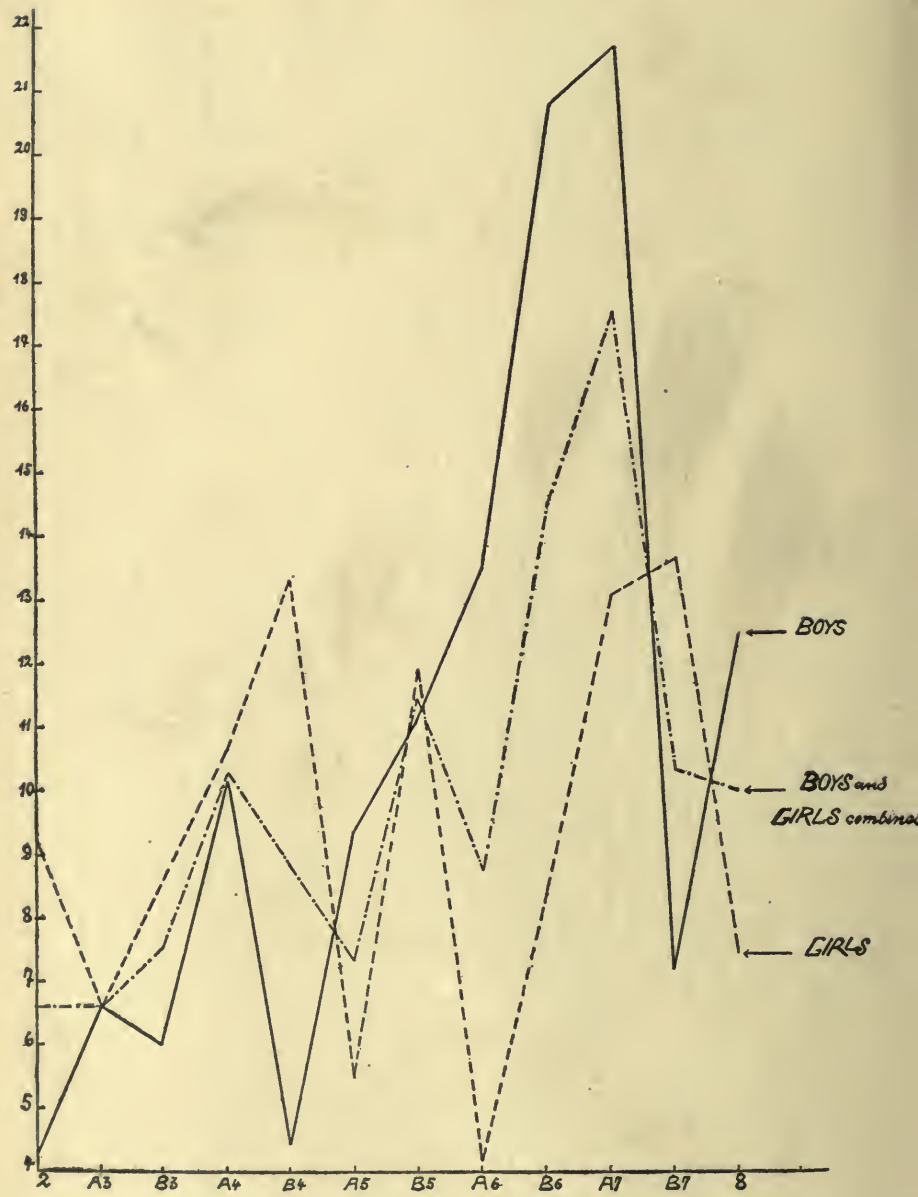


PLATE F. Varying degree of preference arising from difference of the *ideas* conveyed by the words offered. The ordinates indicate the relative strength of such preference; the abscissæ, the school-grades. (From Table VI.)

TABLE VII

SHOWING, IN PERCENTAGES, THE CHANGE OF PREFERENCE FOR A WORD IN A SPECIAL POSITION

The preference is for first words except where the mark (2) appears

List of Time and Space Words

Grade	2	A3	B3	A4	B4	A5	B5	A6	B6	A7	B7	8
Boys	3.2(2)	16.8	6.8	9.6	27.8	3.6(2)	1.4	16.2	8.8	9.	12.8	22.2
Girls	12.6	26.8	6.4	4.8	20.4(2)	.2	3.6	21.8	42.2	16.2	11.	16.6
Boys and girls combined	7.9	21.8	6.6	7.2	24.1	1.9	2.5	19.	25.5	12.6	11.9	19.4

List of Words Denoting Activity and Passivity

Boys	13. (2)	12.	8.6	5.8	28.	2. (2)	3.2(2)	17.6	19.6	12.	22.8	23.2
Girls	3.2	20.6	3.4	2.	2.8(2)	2.8	.6	4.4(2)	46.4	13.	9.8	23.2
Boys and girls combined	8.1	16.3	6.	3.9	15.4	2.4	3.8	11.	33.	12.5	16.3	23.2

List of Words Denoting Dress and Food

Boys	.2(2)	13.8	18.2	16.2	18.4	3.2(2)	19.2	29.6	22.6	9.8	6.8(2)	28.8
Girls	7.8	24.8	11.	3.6	2.4	10.2	9.4	35.	45.	1.2	7.2	12.6
Boys and girls combined	4.	19.3	14.6	9.9	10.4	6.7	14.3	32.3	33.8	5.5	7.	20.7

List of Verbs and Adjectives

Boys	.6(2)	25.	36.6	3.8	28.2	1.6(2)	13.	34.8	18.2	21.	11.6	28.4
Girls	13.8	42.6	21.	12.6	12.4	23.2	13.2	16.4	50.	.2	14.8	11.
Boys and girls combined	7.2	33.8	28.8	8.2	20.3	12.4	13.1	25.6	34.1	10.6	13.2	19.7

Average Course of Preference for a Word in a Special Position in all Lists Combined

Boys	4.3	16.9	17.6	8.8	25.6	2.6	9.2	24.6	17.3	12.9	13.5	25.7
Girls	9.3	28.7	10.4	5.8	9.5	9.1	6.7	19.4	45.9	7.7	10.7	15.8
Boys and girls combined	6.8	22.8	14.	7.3	17.6	5.9	8.	22.	31.6	10.3	12.1	20.8

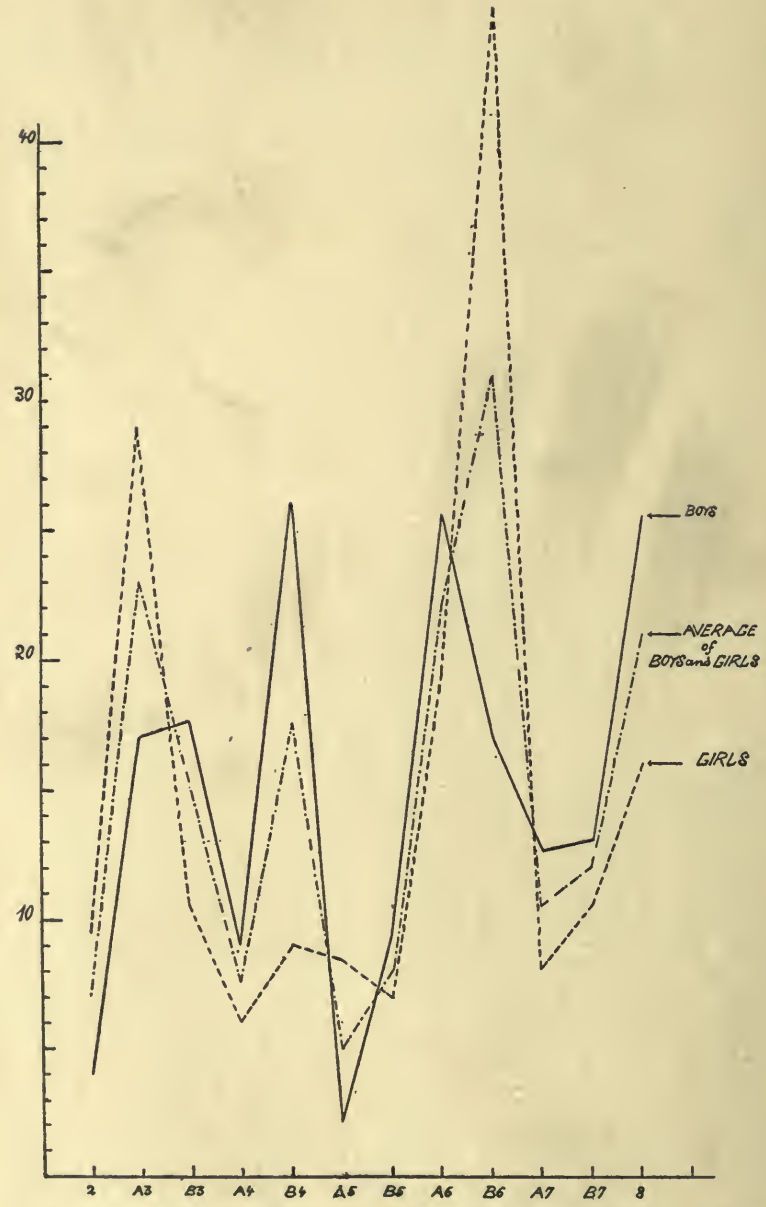


PLATE G. Varying degree of preference arising from mere *position of the word in its pair*. The ordinates indicate the relative strength of such preference; the abscissæ, the school-grades. (From Table VII).

of these grades the curve for preference based upon the order of words is below 10 per cent. There are six grades, in which the curve for preference based upon the order of words is above 10 per cent.; in five of these grades the preference based upon ideas is below 10 per cent. Among the boys, there are six grades in which the curve for preference based upon ideas rises above 10 per cent.; in four of these grades, the curve for preference based upon the order of the words rises above 10 per cent. There are eight grades in which the curve for preference based upon the order of the words rises above 10 per cent.; in five of these grades, the curve for preference based upon ideas rises above ten per cent.

Table VIII

Showing the Degree of Intensity of Preference, in per cent. Computed from Table V which Shows the Average Preference for the Twelve Grades

Time and space words.
boys 7.8 in favor of time.
girls 5.6 in favor of time.
Words denoting activity and passivity.
boys 2.2 in favor of activity.
girls 1.8 in favor of passivity.
Words relating to dress and to food.
boys 16.2 in favor of food.
girls 9.2 in favor of food.
Verbs and adjectives.
boys 12.6 in favor of adjectives.
girls 15. in favor of adjectives.
Average preference for first words in all classes of words.
boys 12.2 in favor of first words.
girls 12.05 in favor of first words.

With the double check in the arrangement of the words, described on page 348, one would expect that where the preference for first words is pronounced, there would be a diminished preference for either class of ideas, and vice versa. And yet, to show that the preference for first words may be most apparent in connection with a preference for the idea conveyed by the word, a copy of two lists of words with actual elections is submitted. The A sixth grade boys' lists for words relating to dress and food are selected because Table III shows a marked preference for words relating to food and also for first words.

As was stated previously, these two sets were written some five weeks apart. The number after each word indicates the number of boys who wrote that word. First words are selected 215 times in the 'second set'; of these, 125 relate to food, 90 relate to dress. In the 'fourth set,' first words are selected 238 times; of these, 157 relate to food, 81 relate to dress.

Table B

A-Sixth Grade: Boys' Elections of Words Relating to Dress and Food

Second Set		Fourth Set		Second Set		Fourth Set	
dress	13	grapes	16	collar	8	cheese	15
grapes	4	dress	2	cheese	9	collar	3
nut	12	cap	11	banana	12	ribbon	6
cap	5	nut	7	ribbon	5	banana	12
hat	11	pie	14	velvet	7	apple	17
pie	6	hat	4	apple	10	velvet	1
berries	12	gloves	10	supper	14	cloak	6
gloves	5	berries	8	cloak	3	supper	12
coat	11	cake	16	silk	9	food	14
cake	6	coat	2	food	8	silk	4
bread	12	skirt	9	beef	12	lace	7
skirt	5	bread	9	lace	5	beef	11
vest	8	meat	16	tailor	9	turkey	17
meat	9	vest	2	turkey	8	tailor	1
fruit	11	shoes	9	pudding	14	necktie	8
shoes	5	fruit	9	necktie	3	pudding	10
stockings	8	potato	15	button	7	carrot	17
potato	9	stockings	3	carrot	10	button	1
butter	12	woolen	9	dinner	14	shawl	6
woolen	5	butter	9	shawl	3	dinner	12

Since other tabulations show the same results as the one submitted, one may conclude that the first word was given the greater preference when it contained the more attractive idea; when the more attractive idea was second, the preference for the first word either was much reduced or disappeared entirely. Although with the majority of pupils it would appear that the two different kinds of preference—the one based on idea, the other based on mere order of presentation—tended now to reinforce and now to offset each other, yet with certain individuals this is not true; they let themselves be influenced solely by position, writing the first word (or the second word) of the entire list of twenty words. Indeed, several wrote the

first (or second) word of more than one list. The following tabulation throws some light on the influence of this factor in the different grades.

Table C

Showing Selection of Words According to Position Exclusively

	Boys		Girls	
Second Grade.				
First word	5 boys	9 lists	5 girls	16 lists
Second word	7 boys	11 lists	5 girls	8 lists
A Third Grade.				
First word	5 boys	10 lists	7 girls	9 lists
B Third Grade.				
First word	1 boy	2 lists	1 girl	1 list
Second word	2 boys	4 lists	1 girl	2 lists
A Fourth Grade.				
First word	3 boys	4 lists	1 girl	1 list
Second word			1 girl	1 list
B Fourth Grade.				
First word	4 boys	8 lists		
Second word			1 girl	1 list
A Fifth Grade.				
First word	2 boys	2 lists	2 girls	2 lists
Second word	1 boy	2 lists		
B Fifth Grade.				
First word			1 girl	2 lists
A Sixth Grade.				
First word	7 boys	17 lists	7 girls	10 lists
Second word			5 girls	8 lists
B Sixth Grade.				
First word	1 boy	2 lists	4 girls	13 lists
Second word			1 girl	2 lists
A Seventh Grade.				
First word	2 boys	4 lists	3 girls	6 lists
B Seventh Grade.				
First word	1 boy	2 lists		
Eighth Grade.				
First word	1 boy	2 lists	6 girls	12 lists
Second word			3 girls	8 lists
Total	42 boys	79 lists	54 girls	102 lists

Table C attracts one's attention to three points:

1. The influence of the factor of the position of the word is more frequent in the lowest two grades, as is to be expected. The excessive breaking out in the A sixth grade is puzzling; yet among the girls, the preference for the first word and for the second word tend to balance. In the eighth grade, one might suspect a conspiracy among the girls, since nine out of ten girls are influenced by the order of presentation. However, it seems improbable that any plan of writing the words

could have been prearranged, as the pupils did not know when the words were to be presented. In fact, they were rather led to think that each presentation was the last.

2. Table C shows that a certain order was followed in one hundred and two lists among the girls and in seventy-nine lists among the boys—giving this factor a greater frequency among the girls in the ratio of 10 to 8. This frequency among the girls is still more pronounced, even when one allows for the greater number of girls' papers. There were 648 girls' lists of twenty words and 600 boys' lists of twenty words—or a ratio of about 16 to 15.

3. This tabulation runs parallel at certain points with the general preference based on the position of the words shown in Table VII. In Table C, in the second grade, the preferences for the first word and for the second word are almost balanced among boys and girls. In Table VII. there is a comparatively small percentage of preference for the position of the word shown in this grade. In the A and B fifth grades, Table C shows little influence of this factor of the position of the word; in Table VII. the percentage of preference is low. In the A sixth grade, one would expect the preference for the first word to more or less balance the preference for the second word among the girls (Table C). But Table VII. shows a marked preference for first words. However, among the boys in that grade the two tables are parallel. The girls of the eighth grade are greatly influenced by this factor of the position of the words (Table C), and there is also a marked preference shown in Table VII. Yet Table VII. gives a high percentage of preference for the position of a word for the boys of the eighth grade while Table C shows almost no influence of this factor. However, the small number of boys in this grade must be considered.

With reference to the first aspect of the larger problem; namely, what inferences may be drawn from this experiment concerning the differences in the affective life of boys and girls of various ages, the results warrant the statement that there is with age, a general, though very irregular, increase in the preference shown for the various classes of ideas.

Furthermore, the preference is more marked among the boys than among the girls. The irregularities of the curves of Plate F may be due to several reasons. At least eight nationalities are represented in these children, and in some of the homes the parents do not speak English. And with a greater number of children the curve would tend to be more regular; 150 boys and 162 girls are the basis for these curves.

It is also noticeable that in some of the grades the children are over age. Ayers[1] gives the following as the normal age:

Grade	2	3	4	5	6	7	8
Age	7–9	8–10	9–11	10–12	11–13	12–14	13–15

The following (to repeat from Table A, p. 348) are the average ages of the pupils in the present experiment:

Grade	2	A3	B3	A4	B4	A5
Age of boys	9.3	10.1	10.9	10.9	12.4	11.3
Age of girls	8.6	9.3	9.3	11.	11.7	12.1
Grade	**B5**	**A6**	**B6**	**A7**	**B7**	**8**
Age of boys	13.9	13.3	13.6	13.6	14.5	15.1
Age of girls	12.1	12.9	14.	12.9	14.	14.8

The table for the normal age is given for the grade; *i. e.*, for the sixth grade, the normal age is from 11 to 13. Any pupil in the sixth grade who is over 13 years of age is beyond the normal age. Or, any pupil in the A sixth grade who is over 12 years of age is beyond the normal age. Glancing at Table A, one finds that the boys in the B3, B4, B5, A6, B6 and B7 grades are beyond the normal age; the girls in the A3, A4, B4, A5, A6 and B6 grades are beyond the normal age.

After studying the irregularities in the curves in Plate F, one finds that in the grades in which the drops occur the pupils are over age. In the boys' curve, the points are the B3, B4 and B7 grades; in the girls' curve, the A3, A5 and A6 grades. Yet, the boys of the B5, A6 and B6 grades are also above the normal age and at these points the curve is steadily rising. The girls of the A4 and B6 grades are also above the normal age and the curve is rising at these points. In the B4 grade, in which the girls are beyond the normal age, their curve has reached one of the peaks.

[1] 'Laggards in Our Schools,' 1909, p. 38.

Considering the second aspect of the larger problem, these curves would have been more comparable with the curves of other studies if the age of the pupils instead of the grades had been used as the basis for the curves. However, some rough comparisons may be of interest. In a 'Study of Children's Reading Tastes' by Miss Vostrovsky,[1] curves are given which show the increase, with age, in definiteness of answer to the question "Why did you select your last book?" The curves show that the boys increase gradually in definiteness with no drops, whereas the girls' increase is not so great nor so steady. There are two drops in the girls' curve—at ten and at fifteen years of age—and a sudden rise from fourteen to fifteen. She infers from the results that boys are more independent in their selection of books than are girls.

In a 'Study of Children's Superstitions'[2] by the same writer, a growth in the critical spirit as children become older is shown. The curves showing the number of superstitions described as untrue by boys and by girls display the same differences between boys and girls as was mentioned in her other study. About the same general difference between boys and girls is shown in Plate F as was shown in Miss Vostrovsky's studies.

In Donaldson's[3] showing of the variation in brain weight during the first twenty-five years, the curves for both boys and girls are far more regular than in my Plate F, and yet there are some points of similarity. From eleven to thirteen years of age, the boys' curves show a steady rise both in the preferences here studied and also in brain weight; then a drop in both to fourteen and then a rise in both to fifteen. Among the girls, there is a decided rise in both from thirteen to fourteen, then a drop in both. Fourteen years is the highest point in both. There is far less similarity between these curves of Plate F and the two curves which Donaldson[4] gives of the changes (A) in the length of the head and (B) in the breadth of the head.

[1] *Pedagogical Seminary*, Vol. 6, p. 523.
[2] In Barnes' 'Studies in Education,' Vol. 1, p. 123.
[3] 'Growth of the Brain,' 1895, p. 105.
[4] 'Growth of the Brain,' 1895, p. 112.

Important curves for comparison are those given by Burk in a study on the 'Growth of Children in Height and Weight.'[1] Two of these sets of curves give the annual percentage of increase in weight (I. J.) and the annual percentage of increase in height (K. L.) of the average American girl and boy. There are several points of similarity between his curves I. J. and my own F. The girls' curves drop from eight to nine years in both; and twelve years, the highest point in I. J., is one of the three peaks in F. However, there is a steady rise from nine to twelve in I. J.; whereas in F. there is a drop and then a rise before twelve years is reached. The boys' curves I. J. and K. L. are similar from nine to fifteen years. There is a drop from ten to eleven in these, comparable to that from A3 (10 yrs.) to B3 (11 yrs.) in F. But, in the curves I. J. and K. L., there is a steady rise from eleven to fifteen, the highest point; whereas in F, there is a drop from A4 (11 yrs.) to B4 (12 yrs.) followed by a rise to A7 (13.6 yrs.), the highest point; then another drop and another rise.

It is thus probable that there is some connection between general physical and mental growth and the development of the affective life, of which preference is an aspect.

[1] *The American Journal of Psychology*, Vol. 9 (1897–98), p. 263.

370

VOL. XVIII. No. 6. November, 1911.

THE PSYCHOLOGICAL REVIEW.

EXPERIMENTS ON HABIT FORMATION IN DEMENTIA PRÆCOX[1]

BY GRACE HELEN KENT

INTRODUCTORY

The population of our hospitals for the insane is made up very largely of cases of dementia præcox. Because of their prolonged residence in institutions, the patients suffering from this disease constitute a public burden of considerable importance.

These patients differ widely in respect to the symptoms which they manifest, and also in respect to their general behavior. Some are efficient workers, and if their activities are properly directed they show a remarkable persistence in the performance of the daily tasks which are assigned to them. Others manifest violent and destructive tendencies, and require almost constant watching on the part of the attendants. Still others show no active tendencies, either useful or harmful, but remain most of the time in a passive state; patients of this type may be seen in large numbers in any asylum, sitting idle about the wards and taking little notice of their surroundings.

That many demented patients possess the ability to perform useful labor is well known, and in public institutions it is customary to assign regular duties to such patients as can readily be induced to work. But, so far as the writer is aware, no attempt has been made to determine experimentally the capacity of dements to receive instruction. The possibility of substituting industrious habits for idle or destructive habits is an open question. It is commonly believed that occupation

[1] From the Psychological Laboratory of the Government Hospital for the Insane, Washington, D. C.

is beneficial to the patients themselves, and it is certain that the regular employment of these patients at some useful occupation would reduce the cost of their care. It is important, therefore, both in therapeutics and in economics, to determine what can be done to further the development of patient labor.

With this in view as the ultimate aim, this investigation was undertaken, the purpose being to show to what extent demented patients possess the ability to profit by experience in the performance of simple tasks. The methods adopted for this study were similar to those used by other investigators in studying the effects of practice; it was necessary, however, to simplify these methods very considerably, in order to adapt them to the ability of the subjects.

In most of these experiments the plan was followed of allowing the subjects a considerable range of freedom in the selection of a mode of procedure. The aim has been not so much to produce certain habits as to determine what habits will, under certain conditions, be spontaneously formed. This study will serve as the introduction to a further investigation, in which the primary aim will be to study the effects of definite and systematic training.

Experimental

1. *General Conditions of Experimentation*

Eighteen female patients served as subjects in these experiments. Forty patients[1] were examined in order to determine roughly their willingness to be experimented upon, and those were selected who, at the first interview, gave the best promise of coöperation. Those who could not be induced to perform a simple act according to instructions were eliminated, on the ground that it seemed inadvisable, in the early stages of the experiment, to spend a large amount of time working with patients from whom no coöperation could be expected.

Five experiments were undertaken, each comprising a series of about twenty tests upon at least six subjects. The intervals

[1] These patients were selected for me by Dr. Mary O'Malley as being fairly typical cases of dementia præcox. I wish here to express my gratitude to Dr. O'Malley for her cordial coöperation in this investigation.

between consecutive tests were usually two or three days, each subject being tested three times a week, on alternate days, for a period of several weeks; the regularity of the intervals was, however, occasionally disturbed by the illness of a subject. The tests were conducted in two of the hospital wards, a small bedroom in each of these wards being set apart for the purpose. Ordinarily the door was closed during the test period, but it was regularly left open during experiments upon patients who were considered dangerous.

The plan was to continue each series of tests upon each subject until the limit of practice improvement should be reached or at least approached, but in most instances it was impossible to carry out this plan. Some of the subjects found the tests irksome, and it became increasingly difficult, as the series progressed, to secure their coöperation. The tasks which were assigned to them possessed little or no intrinsic interest, and no external reward was offered for their performance; there was, therefore, no incentive present beyond the desire to gain the approval of the experimenter. During the first few interviews certain of the subjects frequently asked: "If I do this for you, will you ask the Doctor to let me go home?" It would appear from such questions that the hope of a reward served in some cases as an incentive to undertake the tasks, but great care was taken to avoid encouraging such hope, and there is nothing to indicate that it figured except during the first week, or at most, two weeks. Questions as to the purpose of the experiments were not encouraged, but occasionally, in response to a patient's inquiry, the explanation was given: "I want to see how quickly you can learn to do this, without making a single mistake."

In all these experiments the speed of the performance of an act constituted the principal, if not the only, recorded variable. An attempt was made to eliminate all other variables as far as possible, in order to simplify the treatment of the results. A stop watch was used for measuring the time, but no account was taken of fractions of seconds. As few of the subjects could be taught to begin the work at a signal, they were allowed to begin when they pleased, and the watch was started accordingly.

2. *First Series*

The first three experiments were conducted in the receiving ward, eleven patients serving as subjects. All were patients of comparatively recent admission to the hospital; some, however, were cases of several years' duration[1] at the time of admission. Two were high school graduates, and the others were women of common school education. At the time of the first interview they were quiet and orderly in their behavior, and could be prevailed upon, with more or less difficulty, to comply with any request.

The following notes are taken, for the most part, from the case histories.

Subject 1. Age twenty-seven. Duration two years. Spends her time sewing.

Subject 2. Age forty. Duration one year, convalescent. Obeys any request promptly, but when left to herself sits motionless with bowed head.

Subject 3. Age forty. Duration ten years, hospital residence three months. Dull and apathetic, sits with bowed head, does no work.

Subject 4. Age forty-two. Duration eight years. Busies herself at scrubbing and other rough work. Condition variable.

Subject 5. Age thirty-three. Duration two years. High school education; has held a responsible clerical position. During the period of observation her condition underwent a very considerable change for the worse.

Subject 6. Age twenty-eight. Duration seven years. Condition variable. Busies herself with reading, sewing, and playing the piano. Takes little interest in her surroundings.

Subject 7. Age twenty-five. Duration four years. Condition variable. Usually found sitting on the floor. Does some sewing.

Subject 8. Age thirty-five. Duration seven years. Silent, seclusive.

Subject 9. Age twenty-two. Duration five years. High school education. Spends her time reading.

Subject 10. Age twenty-eight. Duration two years. Works incessantly in the ward, dusting and polishing the floors; very slow in all her movements, and when at work presents the appearance of an automaton. Can rarely be induced to speak.

Subject 11. Age twenty-nine. Duration four years.

Description of Methods. Experiment A.—Rearrangement of digits. Fifteen digits, mounted on blocks of heavy cardboard, 13 by 50 mm. in size, were to be arranged according to a fixed scheme. There was a cardboard frame 36 × 40 cm., which was placed on the table before the subject. A fixed row of digits, 6 2 4 2 0 8 2 3 1 5 3 2 9 7 1, attached to the back of the

[1] The estimation of the duration is only approximate; in some cases it is reckoned from the date of admission to the hospital, and in other cases it is based upon information, more or less reliable, obtained from relatives of the patient.

frame, served as the pattern for the arrangement of the blocks, and they were to be laid in this order along the front of the frame in a space into which they fitted snugly. Before the materials were presented to the subject the movable blocks were arranged in the order 3 2 6 9 8 1 2 0 2 5 2 1 7 3 4, in a space 8 cm. farther from the subject than the space in which they were to be placed. The distance from the space at the front of the frame to the pattern at the back was 38 cm. The subject was requested to arrange the blocks as quickly as possible without making any mistakes.

It will be noted that the difficulty of this task, especially with reference to accuracy, depends largely upon the distance between the pattern and the space intended for the blocks. This test was intended to be so simple that no errors would occur, thus making speed the only variable; it proved to be so for some of the subjects, but not for all. The two rows of figures were too far apart to be compared without special accommodation of the eyes, and it was possible for certain kinds of errors, for instance, the interchange of two blocks, to escape the notice of the subject. Such errors were, however, of such rare occurrence that it has not seemed necessary to make a special study of them, but instead they have been indicated in the curves by interruptions. Other kinds of errors, for instance, the omission of a block, were sure to be discovered by the subject, but could be located only by a careful comparison of the two rows of figures. The time spent correcting such an error was usually quite considerable, and this factor is responsible for some of the deepest fluctuations which are found in the curves. It is evident, therefore, that the method was too complex for the purpose for which it was intended.

Experiment B. Maze.—A copy of the maze used in this experiment is shown in Fig. 1. The subject was given a marker with a blunt point, and was instructed to start from the circle near the center and to trace the way out without crossing a line and without raising the marker. The maze was drawn on tracing cloth, and the marker left no line which would indicate what paths had already been tried, the record of the course being obtained by placing a sheet of copying paper underneath the tracing cloth. The size of the maze was 20 by 15 cm., and the path 1 cm. in width. It is physically possible for a subject who knows the route to cover it in from fifteen to twenty seconds. Speed is the only variable considered quantitatively in this experiment.

Experiments C, C–1, and C–2. Marking Similar Figures.—Eight forms of figures were used in this experiment. Two hundred figures, twenty-five each of eight different kinds,

were arranged by chance upon a page and this chance arrangement was adopted for each of the forms, the relative position of the different figures being identical for the different forms. Form 1 contained eight digits, forms 2 and 3 consisted of 14-point capital letters, and the other forms were made up of geometrical figures. The first line of each of the forms is

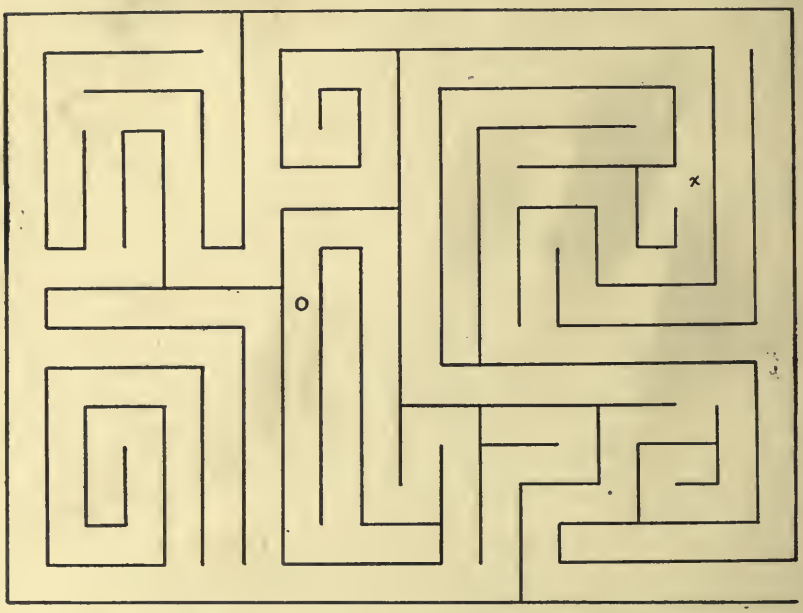


Fig. 1. Maze. Starting point indicated by circle; *x*, see text.

shown in Fig. 2, and the arrangement of the different figures in any form is shown in Fig. 3, which is a reproduction of form 1. The size of the form as printed on the page was 13 by 16 centimeters, one square centimeter being allowed for each individual figure.

The subject was instructed to go over the page as rapidly as possible, marking all the figures of a certain kind, as, for instance, the 2's in form 1, the B's in form 2, etc. At each trial a roughly drawn copy of the figure to be marked was attached to the top of the page. Some of the subjects were instructed to mark the 2's in form 1, and the corresponding figures in the other forms, while others were directed to mark

the 3's in form 1 and corresponding figures in the other forms; but for the individual subject the positions of the twenty-five figures to be marked were identical for all the different forms throughout the series.

In order to determine the skill of the subjects in finding the figures unaided by practice, a preliminary trial was given each subject with each of the forms. After this each subject was given a practice series of about sixty trials with one or another of the first three forms (Experiment *C*); then each subject was given a shorter series with one or another of the forms of geometrical figures (*C*–1); and finally, for comparison with

FIG. 2. First lines of the forms of figures used in experiment *C*.

the preliminary trials, each subject was given a trial with each of the forms which she had not seen during the practice series.

Three trials were regularly given at each interview, making nine trials to the week; during the last week of the practice series and for all subsequent tests, the trials were crowded more closely together, sometimes as many as twelve being given at an interview.

The subjects were instructed to take one line at a time, beginning at the top of the page, to move the pencil from left to right pointing to each figure, and to finish that line before marking any figures of the next line. Great effort was made to induce the subjects to follow these directions, but in some cases it was entirely unsuccessful; the subjects who persistently deviated from the approved mode of procedure were not excluded from the experiment, however, unless the deviation was so marked as to greatly vitiate the results.

Results. Experiment A. Rearrangement of Digits.—The results of this experiment are given in full in Table I., and are also expressed in curves in Fig. 4. Each figure in the table and each point in the curves denote the time record in seconds

4	3	8	5	9	3	4	5	2	4	7	6	
5	8	7	2	3	6	6	3	7	6	3	9	3
8	6	3	7	7	9	3	5	6	4	8	8	
9	4	6	8	9	4	7	9	7	8	4	8	9
5	9	8	5	3	2	8	6	9	8	3	9	
7	2	8	7	6	4	3	8	5	8	5	7	9
2	5	7	2	8	2	8	7	9	4	3	7	
6	2	8	3	7	9	7	5	3	5	5	6	2
7	9	4	9	2	2	4	8	4	2	3	7	
4	5	7	6	3	9	3	6	5	5	9	5	4
7	2	6	2	9	9	4	2	9	7	8	7	
2	8	3	5	6	4	7	8	7	2	4	3	8
2	6	6	5	9	4	5	8	8	3	2	4	
8	2	6	2	3	3	4	4	6	2	5	2	3
4	5	2	5	7	5	4	6	4	3	3	9	
5	9	6	2	9	5	6	6	4	9	6	6	7

FIG. 3. Order of arrangement of figures in the forms used in experiment *C*.

for an individual trial in arranging the digits. The time records of erroneous trials are indicated in the table by asterisks, but are omitted from the curves, the resulting spaces being filled in by broken lines. Any irregularities in the intervals between

trials are shown in the table by the dates,[1] but no account is taken of these in the curves.

TABLE I

EXPERIMENT *A*.—REARRANGEMENT OF FIFTEEN DIGITS. INDIVIDUAL TRIALS
Unsuccessful attempts indicated by asterisks

Subject	1	2	3	4	5	6	7	8	9	10	11
Date	Sec.	Sec.	Sec.	Sec.	Sec.	Sec.	Sec.	Sec.	Sec.	Sec.	Sec.
Oct. 14, 15	130	67	138	69		163	64*	144*	120*	92*	64*
" 17, 18	100	40	110	67	48	138	73	?*	143	115	52
" 19, 20	73	50	90	114*	48	80*	61	95	130	157	59
" 21, 22	60	49	62	97	35	129	118	140*	91	170	58
" 24, 25	57	51	114	59	36	134	133	141	81	132	42
" 26, 27	56	42	81	62	27	60	55	108	109	107	41
" 28, 29	102*	49	90	115		47	70*	105	104	121	43
Oct. 31, Nov. 1	90*	36		73	44	105	115	107	88	123	44
Nov. 2, 3	45	44	74	77	42	52	62	87	94	124	44
" 4, 5	60	40	167	50	60	42	84	95	85	146	50
" 7, 8	76	40	64	68	74	107	84	84	193		49
" 9, 10	62	43	55	79	79	113	82	103	118	381*	46
" 11, 12	58	38	102	53	43	74	67	97	116	159	65
" 14	74	34	56	49	54		111	90	93	117	52
" 16	87	30	52	55	78	69	54	131	92	137	41
" 18	43	40	60	58	47	127*	83	83	114	122	52
" 21	47		57	50	115	52		102	101	161	47
" 23	71	35	49	52	85	43		169*	80	85	50
" 25	54	32	54	48	47	43	47	99	57	90	37
" 28	69	33	62	57	118	50	107	58	101	80	47
" 30	47	33	47	66	58	54	87	54	71	130	50
Dec. 2	52	33	41			59	61	61	83	120	130*
" 5	48	34	55	55				90		80	48
" 7	50	31	40	48		106	89	61			45
" 9		39	40	56			127	116			
" 12								85			
" 14						144*		44			
" 16						43		75			
" 19						63		86			
" 21						33		69			
" 26						57		52			
" 28						75		56			
" 30								178*			

Experiment B. Maze.—The time records given by seven subjects in tracing the course of the maze are shown in Table II., each number denoting the record, in seconds, of the time occupied by an individual trial. The same records, with the exception of that of subject 10, are expressed in curves in Fig.

[1] The two dates for the first thirteen trials indicate that some subjects were tested on one date and some on the other. The subjects were divided into two groups, and were interviewed some on Monday, Wednesday and Friday, and others on Tuesday, Thursday and Saturday; later in the series the two groups were combined, and after October 12 all the subjects were tested on the same days.

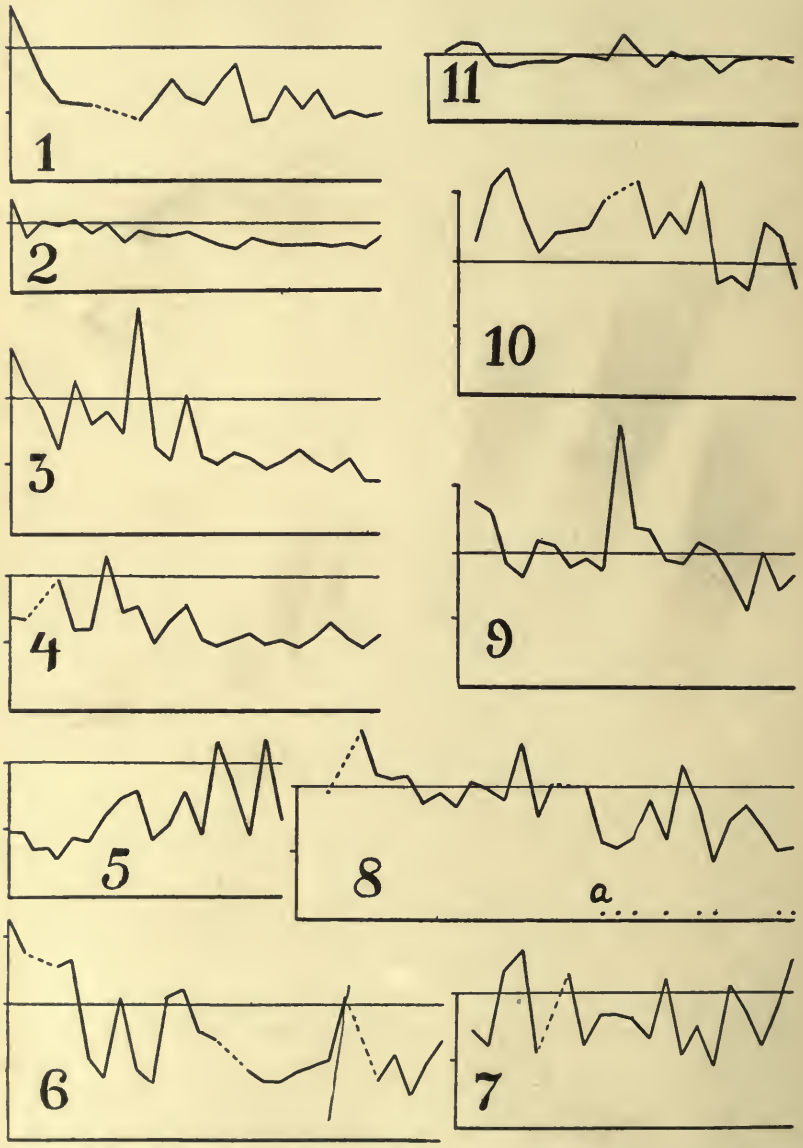


FIG. 4. Experiment *A*.—Rearrangement of fifteen digits. Time curves showing individual records. Broken lines indicate errors. Ordinate scale represents units of fifty seconds.

5. Unsuccessful attempts are indicated in the table by asterisks, and in the curves by interruptions.

Table II

Experiment *B*.—Maze. Time Records of Successful Attempts. Individual Trials
Failures indicated by asterisks

Subject	1	2	3	4	5	6	10
Date	Seconds	Seconds	Seconds	Seconds	Seconds	Seconds	Sec.
Oct. 18				*	83	453	
" 19, 20	399	*	*	640	46	505	*
" 21, 22	300		795	230	42	355	
" 24, 25	210	840	840	118	34	395	810
" 26, 27	73	735	190	202	42	292	680
" 28, 29	123	370	235	139		322	750
Oct. 31, Nov. 1	50	690		104	36	130	767
Nov. 2, 3	37	344	190	124	36	467	*
" 4, 5	39	360	207	204	34	130	923
" 7, 8	44	333	121	340	29	66	
" 9, 10	38	168	79	130	48	158	
" 11, 12	31	106	117	57	31	224	1460
" 14	32	139	102	46	39		
" 16	28	145	85	51	23	369	
" 18	30	247	80	139	28	90	
" 21	18		67	42	30	362	
" 23	21	147	54	69	21	81	
" 25	27	88	57	51	14	117	
" 28	20	149	67	58	20	132	
" 30	19	97	47	47		143	
Dec. 2	22	85	47			104	
" 5			27	60			
" 7		106	49	40		111	
" 9		76	52	40			
" 12		64	50	35			
" 14			49	44		113	
" 16		67	35	72		171	
" 19			41	52		162	
" 21		78	35	35		116	
" 23		66	38	39		113	
" 26		68	40	57		68	
" 28		57		66		120	
" 30		59					

Experiment C. Marking Digits or Letters.—Table III. gives the time and error records in the marking of twenty-five characters, the letter or digit after the number of the subject indicating the character which that particular subject was required to mark. Unless otherwise indicated by figures enclosed in parentheses, each number is the average of three trials, given in quick succession; 0.3 of an error indicates, therefore, one error in three trials. All the errors which occurred in this series were omissions.

These records are expressed in curves in Fig. 6. The solid line indicates the time curve, with the scale at the left; and the dots connected by a light line represent the errors, the scale of the error curve being at the right.

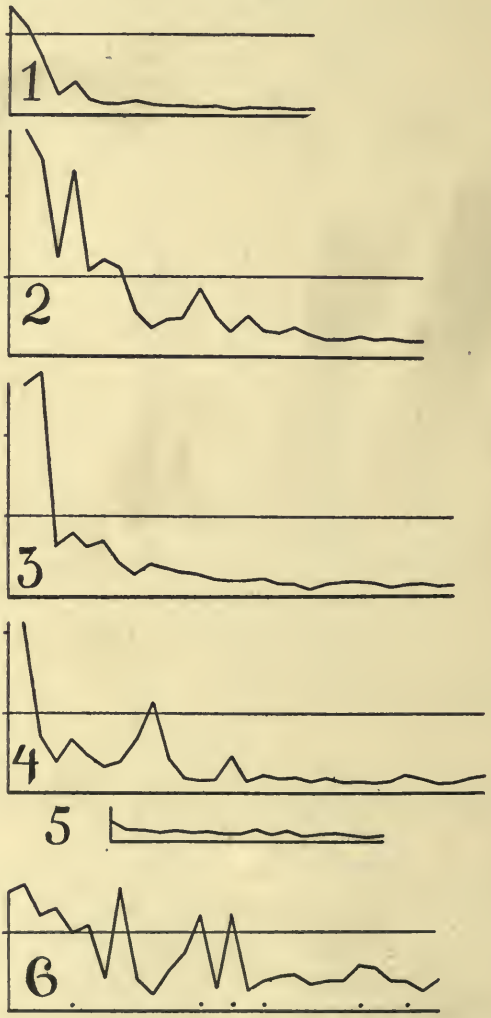


FIG. 5. Experiment *B*.—Maze. Time curves showing individual records. Ordinate scale represents units of five minutes.

The second curve of subject 1 represents a short practice series of tests with form 7. This curve will be found again in

the group to which it more properly belongs (*C*–1); it is placed here also for comparison with the first curve, of which, chronologically considered, it is a continuation.

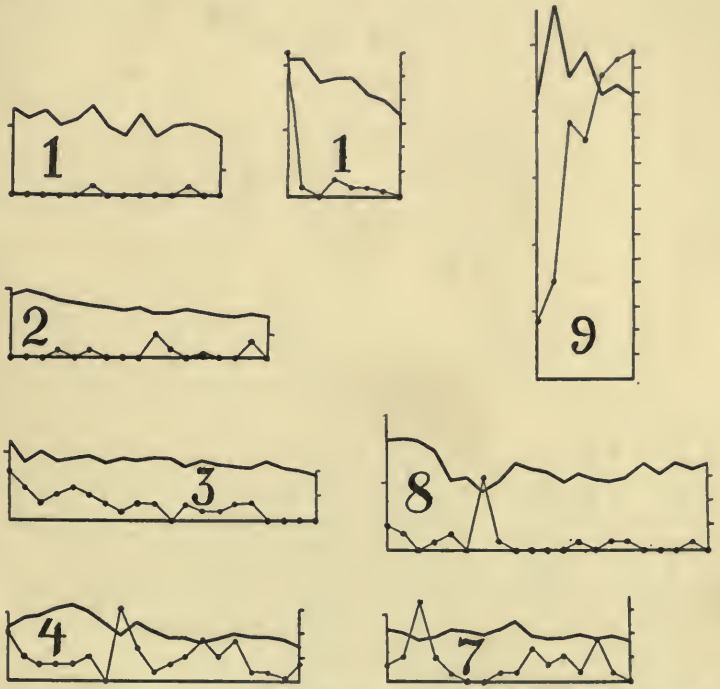


FIG. 6. Experiment *C*.—Marking digits or letters. Time and error curves showing averages of three trials. Time records represented by solid line, errors by dots. Ordinate scale at right represents units of fifty seconds; scale at left represents units of one error.

Analysis of Results. Experiment A. Rearrangement of Digits.—As has already been pointed out (p. 379), this method was favorable to wide fluctuations. The progressive tendency is somewhat obscured by these fluctuations, but the effect of practice is clearly apparent in the curves of subjects 1, 2, 3, 4, 6, 8, 9 and 10, and in the first part of the curve of subject 5.

In order to achieve the maximum of practice improvement in the performance of this task, the subject should develop, early in the series, a definite plan of using both hands. The grouping in multiples of two of the digits which are to be carried in mind is favorable to perfect coöperation between the

two hands. For example: the subject may refer to the pattern and note four digits, 6 2 4 2, take the 6 with the left hand and a 2 with the right, and place them simultaneously; the 4 may then be taken with the left hand or taken with the right and quickly passed to the left, then held while the right hand takes

Table III

Experiment *C*.—Marking Digits or Letters

Digit or letter enclosed in parentheses indicates the character marked by that particular subject. Each number represents average of three trials unless the number of trials is indicated by figures enclosed in parentheses.

Subject and Character	1 (B)			2 (3)			3 (D)			4 (A)			7 (C)			8 (2)		9 (2)	
Date	Sec.	Errors		Sec.	Errors		Sec.	Errors		Sec.	Errors		Sec.	Errors		Sec.	Errors	Sec.	Errors
Nov. 14	64	0																	
" 16	57	0																	
" 21	62	0					58	2.0		39	2.0					81	1.0	213	2.3
" 23	52	0		46	0		43	1.3		47	1.0					83	0.7	281	4.0
" 25	56	0		49	0		50	0.7		49	0.7		39	0.7		81	0	227	11.7
" 28	66	0.3		47	0		44	1.0		54	0.7		37	1.0		73	0.3	246	10.0
" 30	50	0		42	0.3		45	1.3		47	0.7		31	3.3		51	0.7	214	12.7
Dec. 2	44	0		40	0		47	1.0					34	1.0		53	0	222	13.3
" 5	60	0		38	0.3		42	0.7		50	1.0					43	3.0		
" 7	44	0		37	0		45	0.3		42	0.0		39	0.3		51	0.3		
" 9	51	0					43	0.7		33	3.0		37	0		65	0		
" 12	54	0.3		35	0		46	0.7		38	1.3					60	0		
" 14	51	0					46	0		37	0.3					58	0		
" 16	44	0		36	0		39	0.7		31	0.7					51	0		
" 19							43	0.3		32	1.0		35	0		57	0.3		
" 21				32	1.0		41	0.3		29	1.7		39	0.3		55	0	212	13.7
" 23				32	0.3		40	0.7		32	1.0		45	0.3		53	0.3		
" 26				35	0		39	0.7		35	1.7		34	1.3		54	0.3		
" 28				33	0		43	0		33	0.3		31	0.7		64	0		
" 30				31	0								32	1.0		56	0		
Jan. 2				33	0		38	0		33	0.3		34	0.3		65	0		
" 4				32	0.7		36	0	(7)	31	0.1		31	1.7		60	0.3		
" 6			(9)	30	0.1				(11)	25	0.7		33	0.3	(9)	64	0		
" 9						(9)	34	0				(12)	30	0					

a 2, so that two blocks may be placed simultaneously. The digits which are duplicated[1] in the series should be selected in a definite order; after an economical plan of accomplishing the task is decided upon, this plan should be followed con-

[1] It will be noted that the series of digits contains four 2's. This occurred by chance, and when the materials were prepared it was thought to be a trifling matter; it appears probable, however, that this fact had an appreciable effect upon the method adopted by some of the subjects.

sistently at each trial, so that the series of movements will be practically identical for different trials.

Not one of these subjects formed the habit of using both hands systematically, but each followed a more or less crude method of her own. Subjects 3 and 7 performed the task wholly with the left hand, although they were not left-handed; subjects 9, 10, and 11 used only the right hand; the others did most of the work with the right hand, but occasionally used the left for straightening the row of blocks. Subjects 1 and 11 habitually worked from right to left; the others began at the left and worked toward the right. Subject 9 memorized the digits in groups of five, reading them aloud. Subject 10, who used only the right hand, made a practice of drawing two blocks into position, so that she could place them simultaneously with two fingers; she did not lift the blocks, but even when they had to be moved a considerable distance she carefully drew them into place, pushing intervening blocks out of her way with the blocks which she was placing; in case of duplicates she selected the block which was nearest to its correct position. It was in the latter part of the series that this method was observed, and there are no notes to indicate when it was developed. Subject 8 did not develop a definite method until the series was well advanced. At first she selected, apparently at random, any block upon which her eyes fell, and placed it as accurately as she could by sighting its position as compared with the corresponding digit in the pattern, disregarding its position relative to the blocks previously placed. In this way she made many errors, and lost much time correcting them. The first nineteen records, taken as a whole, show negative results: the first successful trial occupied 95 seconds, while the first fifteen successful trials occupied, on the average, 102 seconds, the other four trials being erroneous. At the twentieth trial, which is indicated in the curve by *a*, she began at the left, and placed the blocks in the order 6, 2, 4, 2, etc. In subsequent trials she did not follow this plan consistently, but the notes show that this was the method employed at each of the trials indicated by dots along the base line. It is evident from this that all the progress which

this subject made in learning the task was made after the development of a comparatively systematic mode of procedure.

Experiment B. Maze.—In this experiment, especially in the early trials, the mode of procedure varied greatly with individuals. Some of the subjects made rapid movements, leaving the result wholly to chance, while others worked very slowly and cautiously. Inasmuch as the speed depends largely upon the number and extent of the errors, as well as upon rapidity of movement, the time record gives no indication of the method employed.

Subject 5 worked both systematically and rapidly, making such a good record at the first trial as to leave little room for practice effect. It should be noted that in respect to general intelligence and education she was at a great advantage over any other patient who served as subject in these experiments.

Subjects 1 and 3 worked at first wholly by the trial and error method, moving the marker very rapidly. Subject 2 was extremely diffident about attempting the task, and worked cautiously even after she was perfectly familiar with it. Subject 4 worked rapidly and carelessly, making many errors. Subject 6 worked by trial and error throughout the series, apparently following no plan except to leave nothing untried; she moved very rapidly, and was invariably successful, but in ability to profit by experience she was far behind the other subjects.

The records of this subject are accompanied by numerous notes, and the most significant of these are given in full; the records to which these notes refer are indicated in the curve by dots along the base line.

October 27 (fifth trial). Makes the same errors again and again, with remarkable persistency; tries out the small places faithfully, but when in a long space frequently abandons it in the middle, without looking to see where it leads.—November 16 (thirteenth trial). Knew where to start from, but appeared absolutely helpless as to direction; persistently abandoned the correct path; at one time reached the point eight centimeters from the opening, and then went back to the starting point.—November 21 (fifteenth trial). No evidence that she had any idea what was expected of her; crossed lines freely, and appeared to be trying to follow a line rather than a space; initial instructions had to be repeated. The persistence of a motor habit is very apparent in the error at the point indicated by *x* (Fig. 2, p. 380); she reaches this point many times before she attempts to find anything beyond it.—November 25 (seventeenth trial). Nearly all the time was spent in the upper right corner, where, at the point marked *x*, she repeatedly turned back as abruptly as if she had struck a line.—December 16 (twenty-third trial). No indication from her manner of working that she remembers the form, but she appears to remember what is to be done; started correctly without

instructions.—December 23 (twenty-sixth trial). At the point *A* (indicated in the record) she paused and looked, and appeared to make a definite choice between two courses; chose the incorrect one.

It is clear from these notes that what memory the subject possessed was almost wholly muscular. For a subject who was able to make so little use of past experience, on the visual side, the test was very greatly complicated by the extensive opportunities for forming incorrect habits. The persistence of a given error was the first indication that the subject was forming any habits whatever; in the latter part of the series this tendency was weakened, but was not entirely overcome. In view of the complexity of the task, regarded as a feat of muscular memory, it is significant that the subject was able to give a curve which shows a definite progressive tendency.

Experiment C. Marking Digits or Letters.—Subjects 1, 2 and 3 carefully followed the instructions to cover the page methodically, always finishing a given line before proceeding to the next. The other subjects regularly disregarded all special instructions, and their mode of procedure was regulated wholly by their inclinations. Subject 4 showed a tendency to work mainly in the middle of the page, neglecting the edges. Subject 7 habitually omitted, for a considerable time, one particular figure, but later in the series she showed a tendency to go back to this figure after finishing the rest of the page. Subject 8 gave undue attention to the neatness and uniformity of her markings, and could not be prevailed upon to make a serious attempt to work rapidly; this tendency to draw straight and even lines was observed especially in the latter part of the series. Subject 9 developed a method greatly complicated by mannerisms, and the resulting inaccuracy was so great as to defeat the end of the experiment; for this reason the series was discontinued.

On the perceptual side the possibilities of practice improvement in this experiment were somewhat narrowly limited; the finding of a familiar character, large enough to be immediately recognized and so well isolated on the page as to obviate any confusion with neighboring characters offered very little difficulty. There was more room for improvement on the motor side, and this improvement is shown most clearly in the records

of subjects 1, 2 and 3, the ones who followed the instructions. The others, because of their random methods of working, did not take full advantage of the motor possibilities.

No erroneous markings were made by any subject in this series. Theoretically it might be inferred that the accuracy would vary inversely as the speed, since any subject might, by taking time enough, perform the task correctly. Some of the records would appear to support this view, especially those of subjects 4, 7 and 8; in each of these, it will be noted, the highest point in the error curve corresponds to a point in the time curve which is lower than any previous point. But this is not the case with the curve of subject 1; and the record of subject 9, who took far more than enough time to perform the work correctly, indicates that in some cases, at least, the ratio between speed and accuracy is far from being a constant one. In view of this it has seemed better to treat the two variables separately than to employ any arbitrary method for reducing them to one.

Experiment C–1. Marking Geometrical Figures.—In the case of subject 1, for reasons which will appear later, the practice series with form 2 was brought to a premature close, and form 7 was substituted; the conditions of the tests were preserved exactly as in the first practice series.

The other subjects, at the close of the practice series with the digit or letter forms, were given short series of tests with one or another of the forms of geometrical figures, nine trials being given at a single test period.

Explanation of Table and Curves.—The results of these tests are given in Table IV. The series covered eight experimental periods with subject 1, two periods with subjects 2, 4, 7, and 8, and one period with subject 3. Erroneous markings are separated by the plus sign from errors of omission. These records are represented graphically in Fig. 7. The time curve is indicated by the solid line, and the errors by dots, no distinction being made between different kinds of errors. The interval between two serial days is indicated by a wide space, filled in by broken lines.

Analysis of Results.—It will be noted that, although these

TABLE IV

EXPERIMENT *C*–1.—MARKING GEOMETRICAL FIGURES

Each number represents average of three trials unless otherwise indicated by figures enclosed in parentheses. Erroneous markings indicated by plus sign; all other errors were omissions.

Subject	1		2		3		4		7		8	
Form	7		4		7		4		7		6	
Serial Day	Sec.	Errors	Sec.	Errors	Sec.	Errors	Sec.	Errors	Sec.	Errors	Sec.	Errors.
1	104	6.0	51	3.7	83	4.7	78	11.3 + 0.3	48	6.3	78	7.7
1			53	2.0	74	4.0	71	9.7	47	3.7	86	2.3
1			48	3.3	65	1.3	(2) 97	7.5	42	5.0	91	3.0
3	104	0.3	51	1.3 + 0.7			(4) 54	9.5	52	3.0	89	1.7
3			48	1.3			54	7.7	44	1.3	90	1.0
3			46	0.7			54	8.0	43	1.7	84	1.3
5	85	0										
8	89	0.7										
10	90	0.3										
12	77	0.3										
15	(12) 72	0.2										
17	62	0										

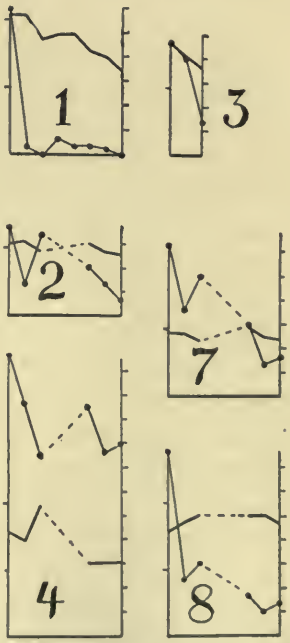


FIG. 7. Experiment *C*–1.—Marking geometrical figures. A point represents the average of three trials. Broken lines indicate interval between two serial days. Time curve represented by solid line, errors by dots. Ordinate scale at right represents units of fifty seconds, and scale at left units of one error.

series of tests were very short, each record shows a practice gain in at least one variable; subjects 1 and 3 gained both in speed and in accuracy.

Three factors combined to make perceptual discrimination more difficult for the geometrical figures than for the letters and digits: in the first place the geometrical figures possessed no clearly defined associative meaning, and no names by which they could be easily and naturally remembered; in the second place, the similarity of the eight figures of a form is much greater than that of letters or digits; and in the third place, they were printed more closely together on the page, so that an individual figure did not stand out so clearly. The difficulty of discrimination for these figures, as compared with forms 1, 2 and 3, was, therefore, very considerable. This difficulty contributed much to the possibility of practice gain.

How much of the actual practice gain can be attributed to the work of the first series could be determined only by further experiments. All that had been gained on the motor side through previous practice was at the disposal of the subjects just as early in the second series as they were able to take advantage of it. No hint was given any subject that the figures to be marked in the new form occupied the same positions on the page as the characters which she had been marking; the subjects were left to discover this for themselves, and it is not known at what stage of the series it was appreciated by them, if at all. In the case of subject 2, the first trial of this series shows a considerable gain, both in speed and accuracy, as compared with the preliminary trial with the same form (Table V.), and this would seem to indicate that the previous work was of some assistance, from the very beginning. In all other cases a comparison of the first trial of this series with the preliminary trial shows a gain in one variable and a loss in the other, which is entirely inconclusive. As the total gain in the first series was slight, the improvement in the second series must be attributed mainly to increasing perceptual familiarity with the figures. This is especially apparent from a comparison of the two curves of subject 1, Fig. 6; after all the improvement shown in the first curve is deducted from that of the second,

the remaining practice effect shown in the second curve is still very striking, especially in respect to accuracy.

Experiment C–2. Preliminary and Final Trials with Different Sets of Figures.—The results of this experiment are shown in Table V. The interval between the preliminary and final trials was about two months, and during this time the particular sets of figures under consideration had not been seen by the subject.

TABLE V

EXPERIMENT *C*–2.—MARKING DIGITS, LETTERS, OR GEOMETRICAL FIGURES

Preliminary and final trials with different sets of figures. Erroneous markings indicated by plus sign; all other errors are omissions.

Subject	1		2		3		4		7		8	
Form	Sec.	Errors	Sec.	Errors	Sec.	Errors	Sec.	Errors	Sec.	Errors	Sec.	Errors
1	99	2			82	1			50	2		
	64	2			75	0			40	4		
2			70	0	62	1	47	0			83	1
			34	1	55	0	34	1			103	0
3	76	3	62	0					87	2	78	5
	47	2	43	0					85	2	107	0
4	85	16	157	2	117	3	140	5+1	122	7	120	10+1
	104	6	65	1	79	1	76	15	76	6	117	8
5	83	2	109	0	129	3			117	5	113	8
	56	2	71	0	74	1			92	1	103	0
6	82	5	136	1	126	1+25	70	5+1	200	7+13	99	4
	108	1	89	1	119	2	64	4	107	1	82	10
7	91	14	125	0	102	4+1			87	3	108	9
	103	6	58	0	112	1			57	6	148	2
8	128	11	132	0	105	4			85	13	142	7
	118	6+1	76	0	104	0			60	7	177	0

The effects of practice transfer may be noted by comparing the preliminary with the final records. There are in all thirty-eight cases of such comparison. Fourteen of these show a gain in speed with a loss in accuracy, and eight show a gain in accuracy with a loss in speed; it is not possible in these cases to balance the gain and loss, but it is noteworthy that sixteen cases show an improvement in both speed and accuracy, while in no case is there a loss in respect to both variables. It is obvious, however, that the gain may have been due in part to general adaptation, as well as to practice transfer.

Notes on Individual Cases.—Subject 1 (see also p. 378). At first this subject showed considerable interest in the experiments, but her attitude toward them was gradually changed when she was transferred to the ward in which the tests were con-

ducted, a less desirable ward than the one which she was required to leave. For a day or two she cried almost constantly, and begged to be excused from the tests. The first interruption in a curve of almost mathematical neatness occurred at this time. (See Fig. 4.) After a week or two the patient adapted herself to the ward, but her interest in the experiments were lost, and throughout the series her coöperation was variable. At the time when experiment *C* was commenced she was in a favorable mood, and showed much interest in the preliminary tests. In the practice series, in which she was required to mark the letter B, she coöperated very reluctantly. The reason for this unwillingness was revealed when the series was well advanced: the subject's mother's name began with B, and she was afraid that the crossing out of the B's would work some injury to her mother. Because of this disturbing factor, the series was brought to a close. The subject was then given a series of trials with form 7, and in this she coöperated better.

Subjects 2 and 3 (p. 378). These subjects gave exceptionally good coöperation in all three experiments, and obeyed with great care all instructions.

Subject 4 (p. 378). An efficient worker in work which calls for large movements, but awkward and erratic in the performance of work which requires care and skill. Extremely inattentive, and frequently paused in the middle of a test to ask irrelevant questions. In marking figures she could not be induced to cover the page systematically.

Subject 5 (p. 378). This patient appeared, at the time of the first interview, to be the most promising subject of the group; but after about two weeks she became disturbed, and grew rapidly worse, until, finally, the tests had to be discontinued. Her time records for the arrangement of the blocks were greatly disturbed by mannerisms; sometimes after placing a block correctly she picked it up, shook it violently, and then replaced it. The effect of such performances is clearly apparent in the fluctuations of the time curve (Fig. 4). She gave much better coöperation in the maze test (Fig. 5); this task was much too easy for a person of her intelligence.

Subject 6 (p. 378). Coöperated willingly, but did not readily comprehend the instructions. Her mode of procedure with the maze has been described above, p. 390. The marked motor tendencies observed in connection with that experiment were apparent also in her procedure with the blocks. At the first trial, because of the great difficulty of making the requirements clear to her, she was permitted to deviate from the plan of the test, and to place the blocks in a space of her own choosing instead of in the space intended for them; this was not permitted again, but the tendency to follow this plan persisted throughout the series. Frequently she appeared to have no idea what was expected of her; at the last trial of the series she started to arrange the blocks indiscriminately, without referring to the pattern, and the object of the test had to be explained as carefully as at the first trial. Only the preliminary tests of experiment *C* were undertaken with this patient. In marking the geometrical figures she gave very little attention to the figure attached to the top of the page, but marked one figure or another, almost indiscriminately. As she finished form 7 (triangles), her attention was called to the figure which she had just marked and she was asked if it were like the one at the top of the page; she replied "It's almost like it—it's a triangle."

Subject 7 (p. 378). Coöperation variable; in the latter part of the series it was extremely difficult to induce her to take part in the experiments; she frequently refused to come to the room, but if brought by the nurse and forced to take her seat she invariably performed the tasks. All attempts to influence her mode of procedure were entirely unsuccessful. Neither of the time curves given by this patient shows any appreciable progressive tendency (Figs. 4 and 6).

Subject 8 (p. 378). This subject coöperated well within certain limits, but she usually ignored any instructions concerning the mode of procedure. As has been noted above (p. 389), her method of arranging the blocks was not developed until late in the series. The experiment was brought to a close by the persistent tendency on the part of the subject to make errors, apparently by intention. No explanation could be obtained. She refused to try the maze. In marking the figures she was attentive to the work, but could not be induced to follow any definite plan of covering the page. She took the greatest care to mark the figures in a certain precise way. She was repeatedly assured that a careless mark would serve the purpose better, because it would require less time; but she showed an increased tendency, especially in the latter part of the series, to give undue attention to the length and direction of each mark.

Subject 9 (p. 378). Coöperation extremely variable. Insisted that the condition of the experiments should be absolutely uniform from day to day, and resented the slightest deviation from the programme. In marking the figures she developed a mannerism which soon became a habit: she made circular movements with the pencil about each figure that was to be marked. At first these circles which she described in the air were just large enough to include the figure, but their size gradually increased until they almost covered the page; as the subject's eyes followed the pencil, she lost sight of the figure which she had spotted to mark long before she was ready to mark it. (See Fig. 6.) All attempts to obtain better coöperation were entirely unsuccessful, and the series was discontinued. The subject did not wish to be dismissed, and later, at her request, she was given another trial; as the mannerism still persisted, no further notice was taken of her requests.

Subject 10 (p. 378). Complied slowly and mechanically with every request. Her mode of arranging the blocks has been described above (p. 389). She performed the maze test successfully a few times, apparently by chance. The time records are given in Table II., but are not expressed in a curve. The subject made the same errors repeatedly. On one occasion after reaching the opening she placed the marker inside again, and went on moving it as before. Her movements did not appear to be directed toward any end, and it was impossible to determine how clearly, if at all, she recognized the opening as the goal to be reached.

Subject 11 (p. 378). At first gave very good coöperation, evidently hoping that the results would prove her to be in good mental health. Gradually, as she began to realize that this was not the purpose of the experiment, she lost her interest in it. Became inattentive, and persisted in talking during the tests. In the latter part of the series it was observed that after placing a few blocks she frequently paused to feel of them, as if to be sure that they were neatly placed, although no readjustment of them was needed.

3. *Second Series*

The seven patients who served as subjects in the second series of experiments were well advanced cases, and were selected as being some of the most hopelessly demented patients who could, with safety to the experimenter, be employed in the experiments. The following notes are taken from the case histories:

Subject 12. Age nineteen. Duration four years. Destructive, untidy.

Subject 13. Age fifty-eight. Duration thirty years.

Subject 14. Age forty. Duration ten years. Noisy, destructive, violent, untidy.

Subject 15. Age twenty-six. Duration five years.

Subject 16. Age forty-five. Duration fourteen years. Noisy, destructive, untidy.

Subject 17. Age thirty-one. Duration eleven years. Noisy, destructive, violent, untidy.

Subject 18. Age thirty-nine. Duration seven years. Idle, indifferent to surroundings, noisy, profane, destructive.

Considerable preliminary experimentation was necessary in order to devise methods adapted to the ability of these subjects. It was only by frequent repetition that any command could be so forced upon their notice as to meet with any response, and even after the performance of a task was fairly started, their attention could be held to it only by constant urging and encouragement. It was essential, therefore, that the task should be extremely simple and short, and that the instructions could be given in a few words.

Description of Methods. Experiment D. Placing Twenty Pegs in Holes.—A kindergarten peg-board and a box of pegs were used in this experiment. The board was a piece of wood six inches square with one hundred holes drilled to a depth of about four millimeters. The greater part of the board was covered, leaving exposed twenty holes, in two rows of ten each, neighboring holes being one half inch apart. The pegs fitted so loosely into the holes that some care was required to keep them from being accidentally displaced. More than twenty pegs were provided. The records consisted wholly of the time occupied by the task. Two trials were given at an interview, making six to the week.

Experiment E. Rearrangement of Ten Blocks. This was a greatly simplified form of experiment *A*. The materials consisted of ten toy blocks 45 mm. square and 17 mm. in thickness, and a wooden frame large enough to hold, with a margin of five millimeters each way, twenty of these blocks in two rows. The blocks were differentiated from one another by squares of colored paper,[1] 29 mm. square, which were pasted diagonally

[1] The following colors of the Milton Bradley Kindergarten papers were used: (1) red, shade No. 1; (2) orange red, tint No. 2; (3) orange, shade No. 1; (4) orange yellow, tint No. 1; (5) yellow, shade No. 1; (6) yellow green; (7) green, shade No. 1; (8) green blue, tint No. 1; (9) blue, shade No. 1; (10) violet, shade No. 2. These colors were easily distinguishable, as was shown by preliminary tests.

upon them. A row of imitation blocks along the front of the frame furnished the pattern for the arrangement of the blocks, the order of colors in the pattern being 5, 9, 4, 3, 7, 8, 10, 1, 2, 6. Before each test the movable blocks were arranged at the back of the frame in the order 1, 3, 8, 5, 10, 2, 4, 7, 6, 9, and the subject was instructed to move each block to the space immediately back of the similar color in the row of imitation blocks at the front. Two trials were given at an interview, but they were separated by a trial with the twenty pegs.

Errors were not permitted in this experiment, and the time occupied by the task constituted the only recorded variable. On rare occasions an error was made, but the time was not registered until it was corrected; the subject's attention was called to the error, if necessary, by the warning to be careful, but she was given no assistance in locating it.

Other Methods.—The interviews with these patients afforded favorable opportunities for experimentation by various methods, not primarily for the purpose of obtaining additional data for this study but rather with a view to testing the methods. The mage was used irregularly with some of the subjects. Another experiment was a very simple picture puzzle, made by cutting a picture postal card, mounted on stiff cardboard, into eight approximately square pieces. If a subject failed, after a reasonable time, to put the pieces together correctly, she was given a little assistance and was then allowed another trial. These patients were also given a little work in plain sewing: the hem of a towel was basted, and the work was started for the subject with thread of a given length. A definite amount was marked off as a day's work, and the subject was requested to sew as rapidly as possible; the stop watch was held under cover, so that she would know that the time was recorded. The stitches were afterwards counted. The principal objection to this, as an experimental method, is that the quality of the work cannot be expressed quantitatively. In order to obtain definite results it would be necessary to try the method on a much larger scale, expressing the results not in terms of the number of stitches taken in a minute, but in terms of the number of towels satisfactorily hemmed in a day.

Results. Explanation of Tables and Curves.—The results of experiments *D* and *E* are given in Tables VI. and VII., respectively. All numbers except those designated by asterisks represent averages of two trials. As the same subjects took part in both experiments, the two sets of time curves are given in parallel columns, in Fig. 8.

Analysis of Results. Experiment D. Placing Twenty Pegs in Holes.—The effect of practice is well marked in the curves of subjects 12 and 13, and is appreciable in those of subjects 14

Table VI

Experiment *D*.—Placing Twenty Pegs in Holes

Asterisks indicate records of individual trials; all other numbers represent averages of two trials.

Subject	12	13	14	15	16	17	18
Date	Seconds	Seconds	Seconds	Seconds	Seconds	Seconds	Seconds
Nov. 26	127*	144*	73*	94*	70*	57*	55*
Dec. 1	112*	137*	77*	60*	88*	70*	51*
" 3	100	103	78	66	69	66	44
" 6	92	94	60	92	58	60	40
" 8	109	92	65	65	75	63	42
" 10	92	89	67	54	65	58	44
" 13	91	102	47	69	82	50	46
" 15	83	74	66	62	83	66	50
" 17	90	100	62	63	69	57	49
" 20	96	96	58	59	71	68	58
" 22	79	91	55	65	58	61	47
" 24	91	95	54	58		61	49
" 27	92	86	80	61		58	45
" 29		86	61	169		57	
" 31	91	79	54	132		56	47
Jan. 3	79	99	64	167*		54	48
" 5	76	123	58			61*	41
" 7	70	102	53				51*

Table VII

Experiment *E*.—Rearrangement of Ten Blocks

Asterisks indicate records of individual trials; all other numbers represent averages of two trials.

Subject	12	13	14	15	16	17	18
Date	Seconds	Seconds	Seconds	Seconds	Seconds	Seconds	Seconds
Dec. 3		127			62		42
" 6		96	123	120	45		28
" 8		76	93	36	40	156	25
" 10		62	79	31	37	66	35
" 13		85	36	31	38	46	20
" 15		64	53	32	35	85	28
" 17		71	49	32	28	70	24
" 20		70	47	27	36	92	25
" 22		79	42	29	30	66	19
" 24		76	35	27		67	24
" 27		64	58	29		61	23
" 29		60	44	103		78	
" 31		63	35	75		56	32
Jan. 3		57	50			65	23
" 5	151	71	52			44*	21
" 7	81	92	34*				
" 12	58		43*				
" 14	53						

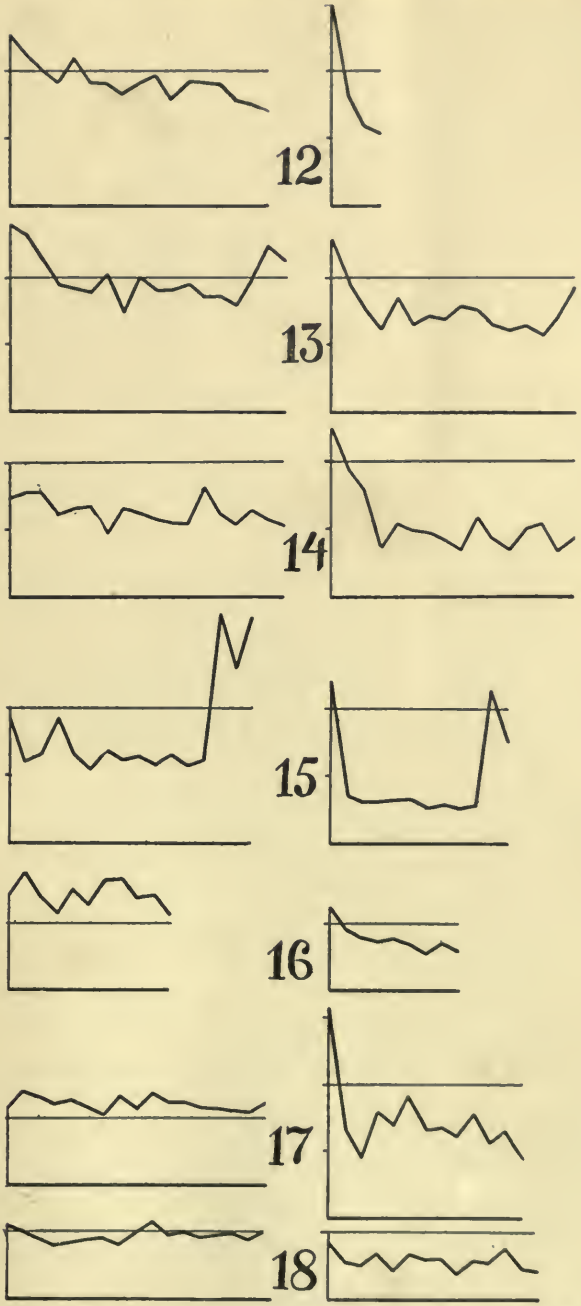


Fig. 8. Experiment *D*, placing twenty pegs in holes, at left of figure; experiment *E*, rearrangement of ten blocks, at right of figure. In both sets of curves the points represent the averages of two trials. Ordinate scale represents units of fifty seconds.

and 15; the other curves are characterized by fluctuations rather than by progressive tendencies.

In this experiment there was comparatively little to be learned which depended upon perception. The general feeling of the pegs, their size relative to the holes, the depth of the holes, and the ease with which the pegs which had been placed could be dislodged—such factors as these could be learned during the first few trials. The possibility of a considerable practice effect depended largely upon the development of a definite method of procedure. The most economical method for the writer, as tested by over fifty trials, is to lay a pile of more than twenty pegs back of the two rows of holes and to work coördinately with both hands, filling first the farther and then the nearer row of holes, and in each case beginning at the left of the row. Another normal observer attained to his greatest speed in a series of ten trials by picking up the pegs with the left hand and passing them to the right, placing them with the right hand, beginning at the left of the board, and carrying the two rows along together. It does not seem probable, therefore, that the same method would serve best for all subjects, but for any subject there is necessarily some method which, if followed consistently, would give better results than could be obtained by random work.

The patients showed no clearly defined tendency to develop a definite method of performing this task. In the early part of the series, it was observed that subject 18 emptied the pegs from the box into the left hand and held them within easy reach of the right hand; but she did not follow this plan consistently, and later in the series worked less systematically. The other subjects usually took the pegs from the box one at a time, taking no care to place the box near the board.

Without the formation of a definite habit of working, the possibilities of practice effect in this task were somewhat narrowly limited; but there was in all cases much room for improvement in respect to attention, and in some cases in respect to the subject's willingness to comply with the conditions of the test.

Experiment E.—All the curves except that of subject 18

show a clearly defined progressive improvement during at least a part of the series.

In this experiment there was considerable to be gained by remembering the position of each block in each of the two series; perceptual familiarity with the materials was easily acquired, and the rapid increase in speed during the first few trials may be attributed largely to this factor. On the motor side also this experiment was more favorable to practice effect than was experiment *D*, for the reason that the movements which constituted the task were more strictly limited. The conditions required that each block be moved from a certain place to a certain place, and although some of the subjects showed a tendency to pick up a block and examine it thoroughly before placing it, still there was a fair probability that the frequently repeated instructions would receive a little attention, and that some of the blocks would be carried directly from one place to the other.

The important matter of deciding in what order the blocks should be moved was left to the caprice of the subjects.

The method which most naturally suggests itself to the subject is to follow the order of the colors in which they occur, either in the pattern or in the row of blocks as they are found. There is an essential advantage in following the order of the fixed series rather than that of the movable series. If the blocks are selected in the order in which they are found (1, 3, 8, 5, etc., or, beginning at the right, 6, 2, 1, 10, etc.; see page 399), either the first blocks must be laid accurately, with some care, or else it will be found necessary later to make occasional readjustments in order to make room for the blocks which are placed last. On the other hand, if the order of the fixed series is followed (5, 9, 4, 3, etc., or 6, 2, 1, 10, etc.), the placing of each block pushes all the previously placed blocks toward the end of the frame, and they are, without conscious effort on the part of the subject, fitted so closely together as to allow ample space for the last blocks.

While the selection of the blocks in the order of the colors found in the pattern is probably the best method which would occur offhand to a subject, experience shows that this method may be considerably improved by taking advantage of certain short-cuts which permit two blocks to be placed simultaneously.

This series of tests was not continued long enough to allow the subjects to perfect their methods if they had been disposed to work methodically; there was, however, only one subject who, at the close of the series, appeared to have made a fair beginning toward the development of a method. Subject 15, during her period of best coöperation, worked from the left

of the pattern to the right, very carefully and systematically, reaching unerringly for the block which she wanted. She worked at a comfortable rate, and made no apparent effort either to increase her speed or to improve her method. (See Fig. 8; for explanation of the rise in the curve near the close of the series, see p. 405.)

No other subject was observed to follow the order of the colors in the pattern. Some took the blocks in the order in which they found them, and some selected them wholly at random.

For some unknown reason, it was more difficult to secure the coöperation of these patients in experiment *E* than in experiment *D*. No subject on any occasion positively and finally refused to place the pegs in the holes, while only three of them, subjects 13, 16 and 18, could at the first attempt be induced to arrange the blocks.

Notes on Individual Cases.—Subject 12. (See also p. 397.) This was the most difficult and unpromising subject with whom the experiments were attempted. At the first trials she seated herself at the table only under compulsion, and it was difficult to prevent her from removing her clothing and pulling out her hair. Every command had to be forced upon her attention by many repetitions. It was only by the most persistent nagging that she could be induced to place the pegs in the holes, but the attempt to obtain a record of this experiment was in no instance abandoned without success. On the other hand, all efforts to induce her to arrange the blocks regularly failed during the first twelve interviews. At the thirteenth interview the blocks were not offered, but instead a puzzle picture of eight pieces was presented; this immediately attracted her attention, and from that moment her attitude toward the experiments was completely transformed. For twelve minutes she worked eagerly at the puzzle, but without making any apparent progress; she occasionally fitted two pieces together correctly, but soon took them apart to try something else. She did not recognize the narrow white border as belonging to the edges of the picture, and there was nothing in her manner of working to indicate that she would succeed by trial and error. She was therefore given some assistance, and was then allowed another trial; she was successful at this and at all subsequent attempts with this puzzle, but did not succeed unassisted with any other simple puzzle which was subsequently offered. At the interview following the first presentation of the puzzle, experiment *E*, the materials of which she had many times refused to touch, was again tried. She willingly, but very awkwardly, arranged the blocks. About a week later all experimentation upon this group of subjects was discontinued, and this subject received only eight trials with the blocks; this series was long enough, however, to show strikingly the effects of practice (Fig. 8). During the last week of observation the subject was given various other tasks, most of them entirely beyond her capacity. She invariably made all possible effort to follow the instructions, and did not once abandon the attempt until required to do so.

Subject 13 (p. 398). It was impossible to hold this subject's attention for any considerable length of time. She showed an irresistible tendency to play with the ma-

terials, and required to be frequently reminded what she was expected to do with them. Her condition was variable; during the last week of observation she was excitable and suspicious.

Subject 14 (p. 398). A very troublesome patient, because of her great psychomotor activity. The following notes are found in the case history, under different dates: "The conduct of this patient is impulsive. She rushes up and down the ward, jumps over chairs, etc. Her movements are all more or less purposeless; she pushes first one patient and then another out of her way, and continues in this restless manner throughout the day. She disarranges her hair and clothing, and presents an untidy appearance. . . . Makes unprovoked assaults on patients; these appear to be more playful than malicious. . . . Is destructive to her clothing, and to furniture and plants on the ward. . . . Patient does no work, is noisy, destructive, violent, untidy." In the experiments her attention was easily gained, but it was only by repeated commands that she could be induced to perform the tasks. She entered the room in a noisy and disorderly manner, and handled the materials roughly, but did not injure them. At the fifth trial with the blocks she intentionally made an error, but finally, in response to a command, she corrected it. When given a towel which was to be hemmed, she took it with many accessory movements, but after the work was fairly started she sewed well and carefully. There was a progressive, although fluctuating, improvement in her coöperation. The maze and the picture puzzle were added to her tasks; she did not succeed with either by trial and error, but was easily taught to perform the tasks. At the eighteenth interview experiments *D* and *E* were omitted, and the maze was presented first. On this day the subject was somewhat excited, and her conduct was more disorderly than usual. She did not frankly refuse to try the maze, but she disregarded all the rules of the test, becoming more troublesome as the minutes passed. The materials were then quietly withdrawn, and the peg board and blocks were presented in the usual order; in these tests she coöperated well, and all accessory movements soon subsided. After those tests she was given a piece of fine sewing, and worked well; when dismissed she left the room in a quiet and orderly manner. It appeared from this that simple and mechanical activities tended to furnish an outlet for the habitual motor restlessness of this patient. She apparently lacked either the intelligence or the interest to undertake tasks which called for voluntary attention, but was willing to perform tasks which could be done mechanically. At the next interview she was invited to remain in the room and hem towels during the tests upon the other patients; she worked well, and did not in any way disturb the tests. The next day, as an experiment, she was sent to the sewing-room, and she has ever since been regularly employed there. At the date of the present writing (six months later) she is reported as being industrious and well behaved.

Subject 15 (p. 398). Conditions variable; at times orderly and industrious, and at other times extremely destructive and violent. (Diagnosed on admission as a case of manic-depressive insanity, but is now considered a clear case of dementia præcox.) At the time when the experiments were commenced, according to the statement of the nurse, she was becoming quiet after a disturbed period. During the first week her coöperation improved with each interview, then for three weeks she was very attentive to the tasks, requiring no instructions nor urging. She was successful in performing the maze experiment; she worked with care and foresight, and made few errors, but did not appear to make any serious effort at speed. The last three records in experiment *D* and the last two in experiment *E* were obtained when the patient was passing into a disturbed condition. At the last three interviews she was extremely troublesome, and it became impossible to continue the series further.

Subject 16 (p. 398). Very troublesome at first, after which, for about two weeks, she appeared to take the tests quite as a matter of course. Later she refused to come to the room, and the series had to be discontinued.

Subject 17 (p. 398). Talked constantly during the interviews. It was difficult to force any command on her attention, but if she could be induced to make a single movement toward complying, she usually performed the task without a pause, talking incoherently all the time.

Subject 18 (p. 398). Motor reactions very prompt; always willing to do anything that was asked of her. Attention difficult to hold; while arranging the blocks she talked constantly about various things suggested by the colors of the blocks, but she worked so rapidly, in spite of her apparent inattention, that it was difficult to observe her method. She sewed very poorly, taking very long stitches, about four or five to the inch. She was successful with the maze; two trials were given daily, and for this reason the results are not comparable with those of the other group of subjects. Six times out of ten the second trial on a given day occupied a longer time than the first. Her success appears to have been due to rapidity of movement, and there is no indication that the fluctuations were determined by any other factor than chance. None of the results obtained from this patient show evidence of practice improvement, and the negative results cannot in this case be attributed to lack of coöperation. The tests were too simple on the surface, for a subject whose motor mechanism was so alert, while in reality they were too complex for a patient of her intelligence. There was, apparently, no difficulty to be overcome which was capable of overcoming.

General Considerations

Casual Observations of Habit Formation.—The readiness with which these patients adapted themselves to the requirements of the experiments furnishes an illustration of the production of habits too complex for exact study. Gatewood[1] has called attention to the persistence in a dement of a habit accidentally formed, even so unnatural a habit as the regular use of the feeding tube. These observations also show the tendency of a habit once formed to persist; two of these subjects (5 and 9) formed accessory habits in connection with the performance of the tasks, and these habits were not overcome. Of greater significance, however, for this study, is the ease with which a new habit may be established, provided it does not come into direct competition with an already existing habit of action.

All the patients gave better coöperation and attention during the second week of the series than during the first week; all complaints and all questions concerning the purpose of the tests tended to diminish, and each of the subjects, for at least

[1] Gatewood, L. C., 'An Experimental Study of Dementia Præcox,' *Psych. Rev. Monograph*, 1909, No. 45, Vol. XI.

a short period, appeared to regard the tests as a part of the regular hospital routine. With ten of the subjects this attitude prevailed throughout the series; subjects 1, 5, 7 and 8 became less disposed, as the series progressed, to submit to the requirements, but they continued, however reluctantly, to perform the tasks. The first attempt to secure a patient's coöperation in a given task was very frequently abandoned as a failure, but only four times throughout the series did any subject refuse to perform a task which she had once performed (subjects 14, 15, 16 and one patient not included among the eighteen).

It should be noted, however, that the efforts of the experimenter to secure coöperation were more obstinately persistent when the success of a series already in progress was at stake than when it was merely a question of deciding whether or not to undertake a new series with a particular patient.

During the first two weeks of the experiments with the first group of subjects, several of them were regularly brought from another ward to the ward in which the tests were conducted, and while waiting for their turn they occupied a seat in the corridor, just outside the door of the experimenting room. Later these patients were transferred to this ward, and for several days they could with difficulty be induced to leave that seat; even subject 1, who was considerably disturbed by the change, remained in that part of the corridor for some days, and was regularly found there waiting for her turn.

Several subjects showed a tendency to resent the most trifling deviations from the routine of the test period, such as a change in the order in which different tests were presented. Subject 4 was greatly displeased when she was called away from her daily work of scrubbing the floor. Subject 2 was considerably disconcerted by the request to perform the tasks in an unaccustomed place. On one occasion, subject 9 was called to the experimenting room ahead of the patient who had preceded her at the last few interviews; she protested that it was not her turn, and when asked what difference it made replied that everything should be done in order. Another time she said, "I can't take my lesson today, because I didn't have my bath this morning; the bath has to come first." She was with difficulty persuaded to perform the tasks, and gave a very poor

record. Both this patient and also subject 3 were displeased when the experiments were discontinued.

In view of the difficulty of controlling minutely the conditions of the daily occupation of patients, excessively servile subjection to routine is not to be encouraged. But just as some children submit more cheerfully to the regulations of the clock than to arbitrarily imposed decrees, so some patients can be governed with the minimum of friction by taking advantage of their respect for order and precedent. The tendency of a patient to demand strict uniformity in the regulation of his daily life is a factor which may well be taken into consideration in connection with any attempt to determine for what occupation the individual patient is best fitted.

Methods of Learning.—With a few exceptions, these subjects made extremely hard work of their light tasks. Their mode of procedure presents a striking contrast to that of Swift's subjects who practised tossing and catching balls. Swift observes:[1] "It is interesting that all the subjects improved by hitting upon better ways of working without any further selection, at first, than the general effort to succeed. There seems to be a competition of methods. . . . Consciousness discovers modes of action already in use and selects some of them for survival because of their success. They then pass into the automatic." This competition of methods was but barely observed among the subjects of these experiments; in many cases the method which chanced to be used at the first trial was followed in all subsequent trials, no attempt being made by the subject to discover a better method. Not one of these subjects discovered, by the trial and error method, the easiest way of performing a simple and obviously possible task, and the experiments were characterized throughout by a high percentage of needless labor. Even in tracing the course of the maze—the experiment which placed the highest premium upon the exercise of judgment—only two of the eight subjects (5 and 15) showed the least tendency, at the first trial, to work rationally, the others working wholly by random methods. It cannot be assumed, how-

[1] Swift, E. J., 'Studies in the Psychology and Physiology of Learning,' *Amer. Jour. Psych.*, 1903, XIV., p. 216.

ever, that an absurdly unsystematic method of performing a simple act is characteristic of mental disease; it may be accaccounted for in part by other factors, such as general intelligence, occupation, and sex.

Some patients who readily accept instructions concerning what is to be accomplished persistently reject all directions or suggestions as to how it is be to accomplished. To induce such patients to follow a given method is a special problem. In this study all attempts to hurry a patient who was naturally inclined to be slow and all attempts to induce a careless person to do accurate work were entirely unsuccessful, and the question as to the possibilities of systematic training remains for further investigation.

It is of special significance that some of the most clearly defined practice effects shown by these results were obtained from patients who were at least forty years of age (subjects 2, 3, 4, 13, 14 and 16).

The methods employed in this study were more favorable to improvement in perceptual discrimination than to the establishment of definite habits of action. In spite of this fact, however, unmistakable practice effects were obtained from a subject whose memory for the experiments appeared to be almost wholly muscular (subject 6, Figs. 4 and 5).

Conclusions

Definite practice effects can be obtained, by means of a short series of tests, from advanced cases of dementia præcox.

The willingness of the subject to coöperate constitutes an important variable. The means employed for obtaining the coöperation of insane subjects must be adapted, to some extent, to the individual.

The importance of giving a patient special training as to the proper method of performing a given task is clearly indicated by the failure of these subjects to devise economical methods.

Practice effect gained in one kind of work appears to be to some extent transferable to another kind of work which differs from the first in its perceptual, but not in its motor aspects.

In any attempt to establish a new habit in a patient, it is well to take advantage, as far as possible, of habits already present.

There are indications that some of the energy ordinarily expended in mischievous performances may, by careful training, be directed into more favorable channels (subject 14). These findings offer some promise that results of practical value, both economic and therapeutic, may be obtained by an extension of this investigation.

SUPPRESSION WITH NEGATIVE INSTRUCTION[1]

BY H. S. LANGFELD

Tests with Alcohol and Caffeine and on Cases of Dementia Præcox and Manic Depression

In a previous article[2] the nature and development of the act of suppression with negative instruction in association tests under normal conditions are described. The object of this paper is to give the effect of alcohol and caffeine upon the power of association and reproduction as well as upon the act of suppression. Introspective data upon the process of suppression is also included. The results of the association tests with suppression in cases of dementia præcox and manic depression will also be briefly described at the close of the paper. The same conditions as in the previous experiment prevailed for these experiments. Simple pictures representing a single object were shown and the subject told to give the first word suggested by the picture. As two of the subjects figured in both series new pictures, but of the same nature as the others, were used, except in the case of the tests on the insane, where the old pictures were employed. In all 120 pictures were used, 10 of which were shown on each test day. With few exceptions a test was made every week. The previous instructions were altered so that the subjects were told not only not to name the picture, but not to name any part of it. This was found necessary because one of the subjects got 'set' to name a part of the picture. Except in the case of the insane subjects full introspection on the main period was required at all times. After the experiment had been some weeks in progress Professor Titchener's suggestion[3] in regard to introspection upon the fore-period was followed. The reaction time was taken with a

[1] From the Harvard Psychological Laboratory.

[2] 'Suppression with Negative Instruction,' *Psychological Bulletin*, June, 1910, Vol. VII., pp. 200–208.

[3] E. B. Titchener, 'A Text-Book of Psychology,' p. 460.

Table I

Tests with Alcohol

Subject A

Al = alcohol test, N = normal test, A = association test, R = reproduction test, R.T. = reaction time, k′ = any kinæsthetic imagery, k″ = kinæsthetic imagery of the name of the picture, v = visual imagery, a = auditory imagery, S.S. = successful suppression, S.R. = successful repetition.

	No.	A		R		A				R				A	R
		R.T.	m.v.	R.T.	m.v.	k′	k″	v	a	k′	k″	v	a	S.S.	S.R.
N	1	1.7	.30	1.7	.16	4	2	6		2	1	2		10	8
Al	2	1.6	.36	1.3	.16	5	2	4				1		9	8
N[1]	3	1.2	.09	1.0	.13	3	1	6		4		6		9	7
Al	4	1.1	.15	1.1	.10	10	4	5		6	3	7		8	10
N	5	1.7	.58	1.3	.16	6	3	6		4	1	3		8	9
Al	6	1.5	.24	1.1	.10	6	2	11				7		10	9
N	7	1.6	.16	1.5	.20	5	1	14		3	2	6		10	9
Al	8	1.3	.16	1.2	.10	9	4	5		1	1	1		9	8
N	9	1.6	.14	1.3	.28	15	3	8	1	3		2		10	6
Al	10	1.4	.46	1.1	.16	4	1	2		1	1	1		10	9
N	11	1.5	.21	1.3	.18	9	4	2		3	1			10	9
Al	12	1.3	.27	1.0	.20	5	3	1		4	3			9	10
N	av.	1.5	.24	1.3	.19	Total 42	14	42		19	5	19		% of un-	3%	18%
Al	av.	1.4	.27	1.1	.14	Total 39	16	28		12	8	17		successful tests	8%	10%

[1] Contains only nine reactions.

Subject B

	No.	A		R		A				R				A	R
		R.T.	m.v.	R.T.	m.v.	k′	k″	v	a	k′	k″	v	a	S.S.	S.R.
N	1	1.8	.40	2.4	1.30	15	10	1		7	4			10	8
Al	2	1.7	.26	1.6	.40	16	11	3		15	8	1		7	8
N	3	1.6	.11	2.0	.19	10	6	2		10	7	1	1	9	7
Al	4	1.6	.15	1.5	.37	16	11	1	1	6	5			9	6
N	5	2.0	.72	1.8	.33	10	7	2	1	9	6			8	6
Al	6	1.4	.18	1.1	.22	4	4		4	3	3	2		10	9
N	7	1.7	.36	1.5	.46	13	11	3		6	3			9	8
Al	8	1.9	.44	1.4	.28	12	10	2		7	7			10	8
N	9	1.7	.21	1.5	.26	11	9		1	4	2			8	9
Al	10	1.6	.28	1.2	.22	13	8			5	5			7	8
N	11	1.7	.30	1.4	.29	6	3	2		4	2			10	9
Al	12	1.5	.30	1.1	.10	7	4			7	6			6	9
N	av.	1.7	.35	1.8	.47	Total 65	46	10	2	40	24	1	1	% of un-	7%	20%
Al	av.	1.6	.27	1.3	.26	Total 68	48	6	5	43	34	4		successful tests	14%	17%

stop watch. There were six normal subjects, two figured in the alcohol tests, two in the caffeine tests and two in the tests under normal conditions. These last tests were introduced as a check in order to be sure that the results obtained in the

TABLE II

TESTS WITH CAFFEINE

Subject C

Ca = caffeine tests, N = normal tests

	No.	A		R		A				R				A	R
		R.T.	m.v.	R.T.	m.v.	k′	k″	v	a	k′	k″	v	a	S.S.	S.R.
N	1	4.1[2]	1.1	2.4[2]	.80	8	7	6				2		10	9
Ca	2	2.2	.40	1.5	.10	2	2	7		4	4	5		9	10
N[1]	3	2.2	.40	1.5	.19	4	3	4		2	2	2		9	8
Ca	4	1.8	.33	1.4	.27	4	3	2		4	4	2		8	7
N	5	2.2	.68	1.7	.58	6	6	1	1	3	3			8	8
Ca[1]	6	1.9	.46	1.3	.28	5	5	4		4	4			9	9
N	7	2.2	60	1.4	.18	8	6	5		7	7			9	9
Ca	8	2.0	.34	1.5	.10	5	5	4		4	4			9	9
N	9	1.9	.34	1.3	.14	8	8	3		6	4			9	9
Ca	10	2.2	.56	1.3	.16	4	4	8		4	2	1		10	8
N	11	2.3	.58	1.3	.28	6	4	3						9	8
Ca	12	1.7	.21	1.4	.32	6	6	4		5	3			8	10
N	av.	2.2	.52	1.4	.27	Total 40	34	22	1	18	16	4		% of un-	8%	12%
Ca	av.	2.0	.38	1.4	.21	Total 26	25	29		25	21	8		successful tests	10%	10%

[1] Contains only nine reactions.

[2] Unusually long R.T. due to inexperience and which it was deemed better to omit.

Subject D

	No.	A		R		A				R				A	R
		R.T.	m.v.	R.T.	m.v.	k′	k″	v	a	k′	k″	v	a	S.S.	S.R.
N	1	1.8	.34	1.4	.27	1	1	2	6	1		1	2	10	8
Ca	2	1.5	.14	1.2	.08	12	9	2		3	3		2	9	9
N	3	2.0	.37	1.5	.21	12	9	2		2	1	1		10	9
Ca	4	1.6[1]	.22	1.2	.11	8	7	2		2	2	2		7	9
N	5	1.8	.28	1.7	.44	8	7	7		4	2	2		10	9
Ca	6	2.0	.30	1.7	.45	13	11	4		8	6	1		10	7
N	7	2.3	.50	1.5	.30	15	8	1		2				10	9
Ca	8	2.1	.26	2.2	.50	15	9	6		14	9	1		10	8
N	9	1.9	.27	1.7[1]	.39	13	9			12	11			10	7
Ca	10	1.9	.39	1.8	.50	9	9	3		3	3			9	9
N	11	2.3	.41	1.7	.35	11	8	1		4	4			10	9
Ca	12	1.9	.32	1.7	.32	18	7	2		10	8	1		10	8
N	av.	2.0	.36	1.6	.33	Total 60	42	13	6	25	18	4	2	% of un-	0%	12%
Ca	av.	1.8	.27	1.6	.33	Total 75	52	21		40	31	5	2	successful tests	7%	17%

[1] Contains only nine reactions.

alcohol and caffeine tests were due to the drugs and not to possible differences between the character of the pictures used on drug days and those used on normal days.

Thirty c.cm. of 95 per cent. alcohol in 60 c.cm. of water

were given on alternate test days to the alcohol subjects (Al-subjects). It was taken three quarters of an hour before the test began. This amount of time was required for the alcohol to

Table III

Tests under Normal Conditions

Subject E

Dg = those days on which the same pictures were used that were used for the foregoing subjects on drug days. The averages of these days are taken just as if they were drug days.

	No	A		R		A				R				A	R
		R.T.	m.v	R.T.	m.v.	k′	k″	v	a	k′	k″	v	a	S.S.	S.R.
N	1	1.5	.26	1.3	.21									7	6
Dg[1]	2														
N	3	1.3	.15	1.2	.24									8	5
Dg[1]	4														
N	5	1.0	.10	1.2	.40	2	2		1	2	2		2	6	6
Dg	6	1.3	.34	1.9	.12	3	3	2		2	2	2		8	9
N	7	1.2	.19	1.1	.26	4	3	1	3	2	2	1		7	8
Dg	8	1.4	.20	1.1	.16	3	2	4		2	1			8	9
N	9	1.5	.36	1.0	.06	6	6		1	2	2			9	7
Dg	10	1.7	.30	1.1	.10	2	2	1	1	1	0			9	9
N	11	1.3	.22	1.1	.16	4	3	3	2	3	2			9	8
Dg	12	1.1	.12	1.0	.10	5	5		1	4	4			8	8
N	av.	1.3	.21	1.1	.22	Total 16	14	4	7	9	8	1	2	% of unsuccessful tests	23%	32%
Dg	av.	1.3	.24	1.2	.12	Total 13	12	7	2	9	7	2			17%	12%

[1] It was impossible to use the data of these days on account of an unforeseen irregularity in the experiment. They would, however, have made no difference in the av. R.T.

Subject F

	No.	A		R		A				R				A	R
		R.T.	m.v.	R.T.	m.v.	k′	k″	v	a	k′	k″	v	a	S.S.	S.R.
N	1	2.7	.88	1.9	.26			3				1		10	6
Dg	2	2.3	.65	1.3	.30	2	2	5	2	5	4	1		8	9
N	3	1.8	.42	1.2	.20	1	1	3	2			3		10	8
Dg	4	1.9	.39	1.5	.23	4	3	5	0	1	1	4	1	9	7
N	5	1.7	.22	1.7	.58	1	1	6		2	1	2		10	6
Dg	6	2.1	.52	1.3	.18	9	4	6				2		10	6
N	7	1.8	.44	1.3	.24	1	1	5		1	1			9	10
Dg	8	1.9	.40	1.6	.30	5	4	7		1	1	3		10	9
N	9	2.3	.48	1.3	.16	3	1	8				1		9	8
Dg	10	1.6	.34	1.4	.10	4	1	7		3	2			10	9
N	11	2.0	.40	1.3	.28	2	2	8						10	7
Dg	12	1.8	.26	1.4	.28			2				1		9	7
N	av.	2.0	.47	1.4	.29	Total 8	6	33	2	3	2	7		% of unsuccessful tests	2%	25%
Dg	av.	1.9	.43	1.4	.23	Total 24	14	32	2	10	8	10	1		2%	17%

show sufficient effect. The tests took place in the morning some hours after eating. Six gr. of caffeine in capsule were given on alternate days to the caffeine subjects (C-subjects). Three gr. were taken one and a half hours and three gr. one half hour before the test. On the other test days capsules of sugar of milk were given in order, as far as possible, to avoid suggestion. A like precaution would have been taken with alcohol if a suitable disguise for the alcohol could have been discovered.

Let us first examine the reaction time for the alcohol tests (Al-tests). The average reaction time for association for all the Al-days shows a decrease of 1/10 sec. as compared with the average time for all the normal days (N-days) in the case of both Al-subjects. With this decrease in reaction time, however, there is less success at suppression. One subject has 14 per cent. failures with alcohol to 7 per cent. without. The other 8 per cent. with to 3 per cent. without.

The C-subjects show a greater decrease in the reaction time for association on C-days. The average for the C-days is 2/10 sec. more rapid than for the N-days. As to the quality of the reaction one subject shows an increase of 2 per cent. in failures, the other an increase of 7 per cent.

In the Al-tests the R.T. for reproduction on the Al-days shows for one subject a decrease of 2/10 sec. and for the other a decrease of 5/10 sec. and in both cases there are slightly less failures to reproduce correctly on the Al-day. There is, then, a decided increase in rapidity to reproduce the former association with, if anything, more successful reproduction on the Al-days.

Caffeine, on the other hand, has no effect on the R.T. for reproduction of either subject. The one subject has 2 per cent. less failures, the other 5 per cent. more.

The N-subjects never show a difference of more than 1/10 sec. between the two sets of pictures either in the R.T for association or for reproduction. In the association test there is a decrease of 1/10 sec. for one and the same R.T. for both sets of pictures for the other. In the reproduction one shows the same R.T., the other an increase of 1/10 sec.

As to the failure to suppress, one N-subject shows no dif-

ference, the other one of 5 per cent., between the two sets of pictures. In the reproduction one shows a difference of 8 per cent., the other one of 20 per cent.

The slight differences in the R.T. of the N-subjects seem to show that the noticeable differences in the R.T. for the other subjects are not due to differences in the material used. The differences in the failures between the two sets of pictures are sufficiently great to prevent any other deduction in the case of the other subjects, than that neither alcohol nor caffeine markedly affect the number of failures in suppression or reproduction. This is well to know in the case of the decrease in reproduction time by alcohol and in the association time by caffeine. The tests have been made on too few subjects for any final assertion to be possible, but the results at least suggest that as much as 30 c.cm. of alcohol do not affect to any appreciable degree a control of one's thoughts or speech such as is necessary in the suppression here required, while that amount of alcohol does increase the rapidity without impairing the accuracy of the reproduction of associations recently made. Caffeine has a quickening effect upon thought. The associations are made more rapidly and the power of suppression is not seriously impaired. This is the effect that casual introspection generally attributes to the drug. It has no decided effect on the reproduction.[1]

[1] Aschaffenburg writes in regard to the effect of alcohol: "Eine qualitative Veränderung der Arbeitsleistung wurde durch den alcohol nicht hervorgerufen." ('Praktische Arbeit unter Alkoholwirking,' *Psychologische Arbeiten*, Bd. I., p. 626.) Ach's observation that alcohol causes an "Herabsetzung der Schnelligkeit und der Verkleinerung des Blickfeldes der Wahrnehmung" ('Ueber die Beeinflussung der Auffasungsfähigkeit durch einige Arzneimittel,' *Psychologische Arbeiten*, Bd. III., p. 288) may in part at least account for the lack of decrease in the R.T. for associations with alcohol, for we know that the motor discharge is aided by alcohol. In the reproduction tests, where the pictures are familiar, the influence of alcohol on the perception is not as great, or not sufficient, at any rate, to offset the increase in the rapidity of the motor discharge. It may be that the decrease in the time for the motor discharge, which, of course, would allow the word to be spoken more quickly, is the sole cause of the decrease in the R.T. for reproduction. For the influence of alcohol on the motor discharge see Ernst Rodem: 'Ueber die Dauer der psychischen Alkoholwirkung,' *Psychologische Arbeiten*, Bd. IV., pp. 40–41. Ach says further that "Caffein bewirkt eine geringe Besserung der Auffasung" (ibid.). This may be a cause of the decrease in the R.T. for associations with this drug. From August Koch's and Emil Kraepelin's observation that "Der Ablauf gewohnheitsmässiger Associationen wird durch das

A record was kept of the different types of images as is shown in the tables. It was thought that the drugs might change the type and it was also considered of interest to determine if the different reaction times could be correlated with the different types. The results seem negative. There is in general little difference between the imagery in normal days and drug days and nothing can be deduced from the differences that do occur since the normal subjects show differences as great. As to the correlation of types of imagery with reaction time for association, there is a tendency for those who have much visual imagery to have longer reaction times. This may be observed in subjects C and E. Subject A, however, is an exception. Subject E, who has the least amount of visual imagery, has the quickest R.T.

An examination of the quality of the reaction words has also been made to see if there is a change in the distribution under alcohol and caffeine. It was thought that although there was slight effect upon the actual suppression of the name of the object, yet the quality of the reaction words might perhaps give a clew among other things, to the difficulty of suppression with and without the drugs. A classification was chosen which was expected to bring out any such change in distribution. An increase in the number of descriptive words or of those words caused by suggestion through resemblance might indicate a difficulty to get one's thought away from the picture. An increase in repetition or a change in the number of super-ordinate words, or in the type of reacting under the influence of a drug, would also be of interest. The decision as to whether a word should be classified under contiguity or coördination is, as is well known, often a difficult one. Without introspection it is always a guess.

Turning to the table we find a surprising similarity in the distribution of the words on drug days and normal days. There is, however, one difference of interest. Both caffeine subjects, *C* and *D*, have fewer words from suggestion on C-day. C has a change from 7 to 2 words and D one from 18 to 4.

Caffein erleichtert" ('Ueber die Wirking der Theebestandtheile auf körperliche und geistige Arbeit,' *Psychologische Arbeiten*, Bd. I., p. 488) one would expect that caffeine would quicken the reproduction. This was not the case in our experiments.

Table IV

Classification of Association Words

	Subject A			Subject B			Subject C			Subject D			Subject E			Subject F		
	Al	N	Total	Al	N	Total	Ca	N	Total	Ca	N	Total	Dg	N	Total	Dg	N	Total
Descriptive .	2	2	4	8	6	14				1	1	2	3	2	5			
Verb.....	32	35	67	4	1	5	1	1	2	1		1	16	9	25	43	42	85
Contiguous .	14	15	29	25	24	49	41	37	78	18	23	41	3	4	7	1	4	5
Super-ordinate ..				1	3	4				3	1	4	1		1			
Coördinate .				4	4	8	3	4	7	18	13	31	4	2	6			
Repeated...	6	2	8	1	1	3	2	1	3	4	1	5	2	5	7	6	7	13
Suggested ..		2	2	3	2	5	2	7	9	4	18	22	3	8	11	3	3	6

As will be seen there is a tendency for the subjects to react always with a word of the same class; that is, they become stereotyped in the quality of reaction words. In the previous work[1] we mentioned this tendency of one of the subjects to react with verbs. In the present experiments we find two distinct verb types, A and F, and one fairly marked verb type E, two contiguity types, B and C, and one type, D, in which most of the words are divided between contiguity, coördination and suggestion. It is interesting to note that the subject E, who became the least stereotyped, also has the most failures to suppress. We will return to the discussion of these failures later. Subject B, who has the most descriptive reactions, also has a comparatively large number of failures to suppress, which fact in some part bears out the hypothesis that reaction by descriptive words shows a certain difficulty in getting away from the name of the picture. There is no correlation between these types and the reaction times.

Unfortunately the attitude of the subjects in the fore-period was not ascertained at the beginning of the experiment, so that it is impossible to give an account of the development of the consciousness of the instruction during this period. Later, however, the contents of consciousness during the fore-period were occasionally tapped by interrupting the experiment before the picture was exposed and without warning to the subject. It was found that the negative instruction, which was always repeated by the experimenter at the beginning of each series,

[1] P. 206.

continued to 'ring in the ears' of the subjects, during the fore-period of the first test, or was repeated by the subject in its negative form. As a rule after this first test there was nothing in consciousness pertaining to the instruction. The subjects described this attitude as 'passive.' They sat before the shutter and waited for it to open. This expectancy was all that could be found in consciousness. One subject, E, proved an exception. He repeated the instruction in its negative form before almost every exposure. Sometimes this was reduced to 'No, no,' or 'not picture or any part of it.' Several times it was in the form of 'do what you did before.' This is a positive command, whose purpose is to suggest a negative attitude. Beyond the mere repeating of the words of instruction the only representation in consciousness of the negative attitude was a focusing of the eyes on a point beyond where he knew the picture was to appear. It was in this background that he seemed to himself to search for a word, after recognizing the picture, and he felt that this attitude aided him in getting away from the name of the picture. In almost no instance was the negative instruction changed to a positive one. The words touched off a cortical set, which may be called a negative set, in so far as it has an inhibitory effect either upon the thought of the name of the object or its expression in speech. The nature of this set cannot be described further, but that there are two different processes, a positive and a negative one, seems fairly well established.[1] Although little was found in consciousness beyond the words of instruction to represent this negative set, one may hazard a guess that in the early development of the individual there is a characteristic representation, but that, following the laws of habit the process becomes more or less mechanical and is generally limited to physiological processes.

A full description of the process of suppression during the main period is given in the former paper. These experiments furnish no new data. The introspection does, however, throw some light upon the causes of failure to suppress and failures in reproduction as well as some description of the effect of the

[1] See summary of former paper, p. 208.

inhibitory attitude. As the tables show, there were few failures to suppress. Subject E shows the most. He is also the one subject who repeated the instruction before almost every test. When he failed to suppress he generally said that his attitude was not good. Either he had not repeated the instruction or he hadn't the instruction well in hand. There seems to be strong indication here that the subject lacked concentration. In terms of this problem the instruction not only tended to disappear from consciousness as was the case with the other subjects, but the attitude as represented in physiological processes tended also to disappear, so that the instruction had to be repeated. Like indications of lack of concentration were observed in a different experiment conducted by another experimenter in this laboratory.

When there was difficulty in recognizing the picture, the delayed act of recognition with its affective tone seemed to weaken the attitude of suppression and the picture was frequently named. A possible description is that the name of the object is delayed by lack of recognition, and when this recognition occurs the name comes into consciousness with a bound, so to speak, and as the attitude of the instruction had been pushed aside for the moment by the difficult act of recognition, the name comes to utterance.

An emotional state of one of the N-subjects, who had a momentary difference with the experimenter on one of the test-days, also caused an unusual amount of failure. An occasional unavoidable distraction also weakened the attitude. An attempt was made to conduct similar experiments under conditions of distraction, but it was found that distraction, such as is produced by noise, odors, mental work, or emotional states could not be made subject to the will of the experimenter.

As regards the failure to reproduce the previous association, one of the chief causes is the fact that there were several images in consciousness before the one given in the association test. In the reproduction test that followed, one of these images, and not the one actually given as an association, was then reproduced. For example: association "BRUSH—*hair*. Recognized brush. Kin. image of brush. Active suppression.

Visual of side of horse with brush on it. Then hair." Reproduction '*brush—horse.*' There were also instances of the reproduction of a word that had just preceded instead of the correct word.

Varied effects of the inhibitory process could be observed in the introspection. At times a certain perception was suggested by the shape of the picture. As soon as it was seen not to be the name of the picture, this perception was given as an association, even though the object itself may not have been fully recognized. For example: association "CARPET-SWEEPER—*typewriter.* Started to recognize typewriter. Saw it wasn't. Said typewriter." Association "RATTLE—*hair.* Percept slow in forming. After slight pause hair came. Suggested by picture although I knew it wasn't that." Association "RAT—*dog.* Recognized it wasn't dog. Looked so much like it that I said dog. Then recognized rat." Here is also an example where the name would not come. Associaton "BATH-TUB—*bowl.* Percept came somewhat slowly. Tried to get name but couldn't. Then came word bowl. The form suggested bowl." Even when the association is not by suggestion the name may be held up until after the association, both in association and reproduction tests. Association "BLOCK—*axe.* Axe came immediately. Afterward chopping block," or reproduction "PITCHER—*water.* Water came automatically. Kin. of pitcher afterward."

There are also instances where the inhibitory process instead of inhibiting the name of the object gives it a different meaning, attaching the word to imagery sometimes quite different from the picture. This very frequently occurs with verb associations; for example, association "FLY (noun)—*fly* (verb). Its wings were prominent," or association "BRUSH (noun)—*brush* (verb). Recognized brush. Visual image of brushing." Association "SMALL TENT—*circus tent.* Thought of that tent, then large circus tent." Association "LADDER—*step.* Aud. ladder. Persisted a short time. Made visual image of step to a house. Then said step."

Tests on Insane Subjects

Tests of a nature similar to the above were made upon dementia præcox and manic depression cases at the Danvers State Hospital.[1] A similar instrument and sixty of the cards used in the first series of normal tests were employed. Introspection was not required, inasmuch as there would always be grave questions as to its reliability. The instructions were those of the first normal tests and did not include prohibition to name a part of the picture. There were eight dementia præcox, three manic and one depression patient.

Table V

Tests on Dementia Præcox and Manic Depression

No. = number of tests, F.S. = number of failures to suppress, F.R. = number of failures in reproduction.

Subjects		A Tests			R Tests			F.S.	F.R.	Classification of Association Words						
		No.	R.T.	m.v.	No.	R.T.	m.v.			Des.	v.	Con.	Sup.	Coö.	Re.	Sug.
Dementia Præcox	H	58	3.3	1.56	58	1.5	.33	19	7	1		20	20		5	
	R	51	6.4	3.07	42	4.5	2.14	51	60	6	1	7				
	Ah	58	4.9	2.23	58	1.8	.65	0	7			27	13	3		4
	Cu	47	5.5	2.59	47	1.9	.54	21	9	10	4	10	6		1	
	S	58	3.2	1.56	58	1.9	.66	2	7	4	18	24	3	2		
	M	56	3.7	1.05	56	2.5	.90	0	9		1	43			1	1
	Ch	57	2.1	.76	56	1.1	.25	9	5	2	3	34	2	2		
	T	55	2.8	.92	55	1.3	.23	11	14	7	5	14	7	2		
Depression	N	58	4.5	2.03	58	1.9	.65	5	12	2	16	15	15			
Manic	E	55	2.3	.75	55	1.5	.35	5	5	2	1	34	11		4	
	Al	40	3.4	1.29	40	2.9	2.20	0	7	31	1	6				
	G	39	1.2	.38	39	.75	.13	3	0	3	29	6				

From the table we see that the reaction times for association are in all but two cases longer than for any of the normal subjects—in many instances longer than could be accounted for by a difference in education. These long R.T. occur both for dementia præcox and manic depression. Among the manic depression the manic type is more nearly normal. One showed an unusually rapid reaction time. As regards the R.T. for reproduction in all but three cases it closely resembles that of the normal subjects. Many of the m.v. are extremely large.

As regards suppression, the three cases which show many

[1] The author takes this occasion to thank the physicians of the hospital for their kindness in permitting and facilitating these tests.

failures are all dementia præcox. One has a very great many failures, the others have more than the normal subjects with the exception of normal subject E, who had most of his failures during the emotional disturbance. The one patient with the largest number of failures also has the largest number of failures to reproduce the correct word.

The fact that all the large number of failures to suppress are by dementia præcox patients seems to corroborate the theory that an impairment of the will in which a decrease in attention plays an important part, is a characteristic feature of this disease.[1] The lack of retentiveness in the case of one dementia præcox patient seemed to be a special feature of the case not found in any of the others. In her case the symptoms of dementia were much more marked.[2] A dementia præcox patient not recorded named the object at every exposure. He showed a willingness to take part in the experiment and whether his failure to suppress was due to any difficulty upon his part to restrain from uttering the name or from a negative attitude toward the experiment could not be decided.

These subjects also show distinct types of reaction as regards the quality of the reaction word. Most were of the contiguity type, one was distinctly of a verb type, two showed many superordinate words and one was decidedly of a descriptive word type.

Summary

As was mentioned above, there were too few subjects to permit of any generalization. Several interesting possibilities, however, were suggested by the results.

1. 30 c.cm. of alcohol caused a decrease in the reaction time in the reproduction tests. It did not appreciably affect the suppression or accuracy of reproduction.

[1] Alfred Busch, 'Auffassungs- und Merkfaligkeit bei Dementia Praecox,' *Psychologische Arbeiten*, Bd. V., p. 336.

[2] Dr. Charles Ricksher remarks in regard to the retentiveness of dementia præcox patients, " . . . when cases are arbitrarily classed according to the apathy they show, the duration of the disease, etc., those showing the more marked deterioration almost invariably show less ability to recall either the auditory or visual stimuli than do those with a slighter degree of dementia." ('Impressibility in Dementia Præcox,' *American Journal of Insanity*, Vol. LXVI., No. 2, p. 229.)

2. Caffeine caused a decrease in the reaction time for association and showed no appreciable effect upon the suppression or accuracy of reproduction.

3. Introspection on the fore-period showed no evidence of the necessity of translating negative into positive instruction. This makes it probable that there is a distinct negative as well as positive attitude, which in most instances can be described solely in terms of cortical set.

4. The lack of a power of suppression was found only in some of the dementia præcox patients. The manic depression patients were normal in this respect. Accuracy of reproduction was normal in both dementia præcox and manic depression with one exception (dementia præcox).

THE HYGIENE OF SLEEP

BY E. C. ROWE

This study was undertaken for the purpose of determining if possible whether any *direct* relation exists betweeen the *amount* of sleep and the barometric conditions and also between the amount of sleep and the number of hours spent in mental work. A correlation has also been attempted between the *amount* of sleep and general feeling-tone. For this purpose a daily record was kept of the number of hours and minutes of sleep and also of the number of hours and minutes of work. Of course the time of sleep cannot always be exactly determined for the very evident reason that one cannot be sure just when he went to sleep. This inaccuracy is, however, reduced to a minimum if one falls asleep quickly upon retiring, as was the rule with the subject of the study. A watch was kept at hand so that in case he awakened during the night the time might be noted. The subject was soon able to estimate the time during which he was awake quite closely. This was determined on various occasions by estimating the duration of wakefulness after noting the time of awaking and then comparing the estimate with the actual time that had elapsed. On the whole, he feels confident that the time of actual selep was obtained within an average inaccuracy of ten or fifteen minutes.

The work record was limited to work done in connection with university studies. The feeling record from which the curve is constructed was kept by a second party and was based upon observation and reports by the subject. This curve is naturally less accurate than the others both so far as the *amount* of the deviations from an assumed mean are concerned and also with regard to the actuality of the deviations. This is not, however, to be interpreted as an admission that the curve is untrustworthy. In the main it is correct. One can tell how he feels from day to day, but when one undertakes to express these feeling conditions in terms of a curve slight inaccuracies may

creep in, especially when the feeling conditions are quite similar for several days in succession. It is the writer's judgment, however, after six months of observation, that our general psycho-physical tone changes more from day to day than the average person is aware of.

The record was begun in October, 1907, and continued without interruption up to the spring recess, April, 1908. The sleep record of a two and one-half year old boy living in the same house and under the same general conditions as the adult subject, was kept for the same period for comparative purposes and is correlated with the barometric curve in chart 2.

The objection may be raised that the keeping of such a record is likely to influence one's habits of sleep and work and thus vitiate the object of the study. To concede that it did have some unconscious influence is by no means to admit that such influence destroys or even interferes at all seriously with the significance of the results. While it did here and there lengthen the period of wakefulness this disadvantage was probably more than offset by the subject's extra precaution with regard to diet and general living habits throughout the period. Sleep is probably influenced by a wide range of stimuli and conditions all the way from the condition of digestion, nerves and muscles up to the last psychic process preceding sleep. The addition of one minor factor may be counterbalanced by the suppression of another.

With certain brief exceptions due to illness, the subject's habits of sleep have, so far as he remembers, always been good, going to sleep quickly and sleeping soundly. He has always slept from seven to eight hours, retiring moderately early and arising rather early. Throughout the period for which the record was kept the subject was the only occupant of a large third-floor sleeping room on an unusually quiet street. The room was always flooded with fresh air before retiring and kept cool throughout the night by direct ventilation through an open window. The hours of retiring varied from nine to eleven o'clock. The controlled conditions were therefore favorable to good sleep.

The records were begun on October 22, 1907, and continued

for 162 days. The sleep, work and feeling data were of course recorded each day. The barometric data were obtained from nearby observatories. Probably no better place or time could be chosen in which to study the relation of barometric changes to sleep than a New England winter with all its weather contrasts. Surely if any correlation exists between the two it should come out in the frequent and sharp changes incident to such a winter. Especially should this be true for the two subjects whose sleep records were kept inasmuch as neither one had ever before spent a winter in New England and hence was in no sense acclimated to the peculiarities of local conditions.

The accompanying curves, based upon these records, are, so far as possible, drawn to the same scale. Work and sleep, measured in terms of hours, are constructed upon the same scale, but barometer and feeling are necessarily based upon distinct measurements. But this in no wise renders impossible the comparison or correlation of any one of the curves with any or all the others. Their comparison and possible correlation are here considered from two points of view. First, the curves are compared with each other with regard to the direction in which they move for the successive days. Second, they are compared with regard to the amount of deviation from their respective averages.

In comparing the curves with reference to the direction in which each is moving in relation to another, *three* possibilities are considered, namely, that they may move in the same general direction (though of course not necessarily parallel any or all the way); that they may diverge; and thirdly that they may cross each other. With these three possibilities under consideration the following results are obtained from the adult curve:

1. Sleep and work (Sundays excepted) ..... 133 days.
   (*a*) In same general direction ..... 81 days.
   (*b*) Sleep up; work down ..... 29 days.
   (*c*) Sleep down; work up ..... 24 days.
2. Sleep and barometer ..... 162 days.
   (*a*) In same general direction ..... 78 days.

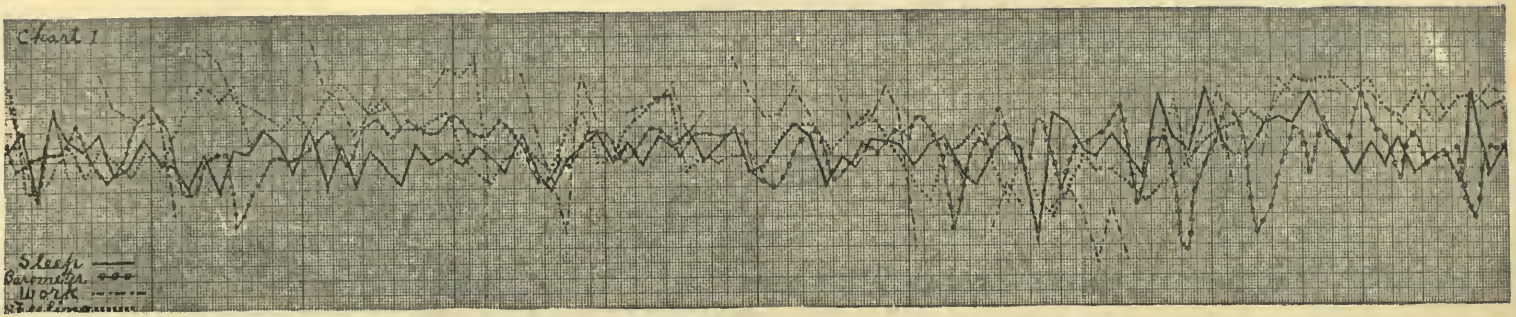


Chart I
Sleep
Barometer
Work

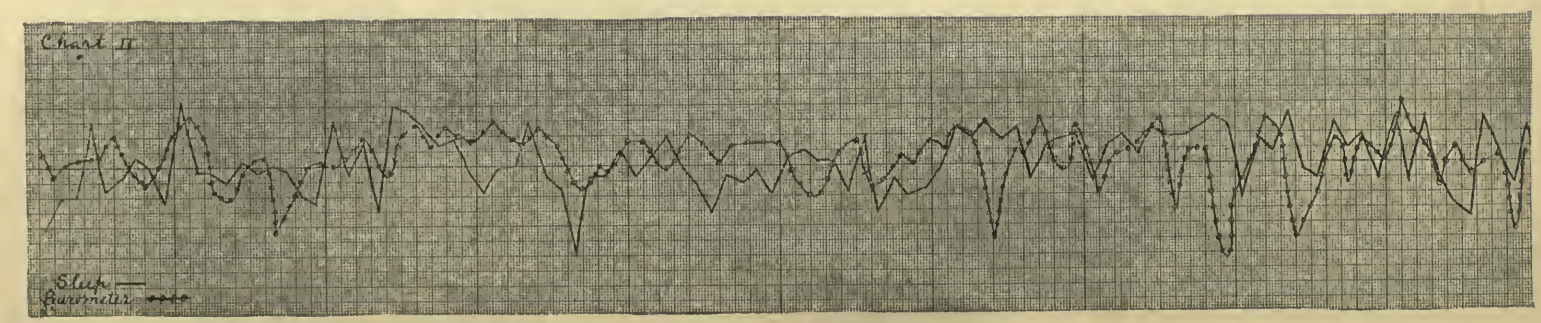
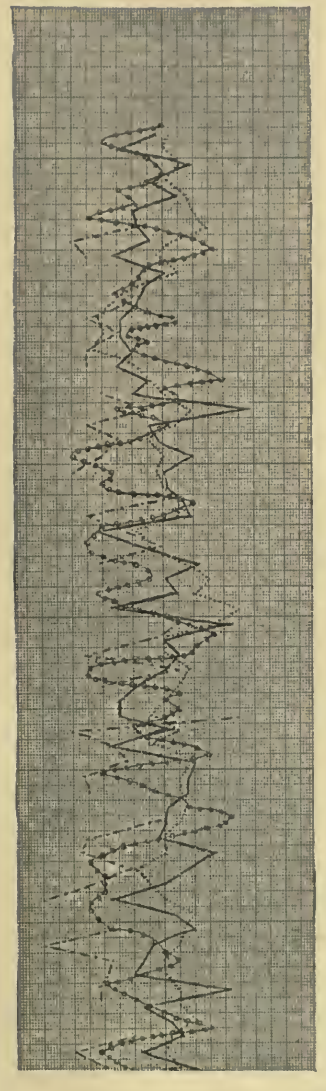
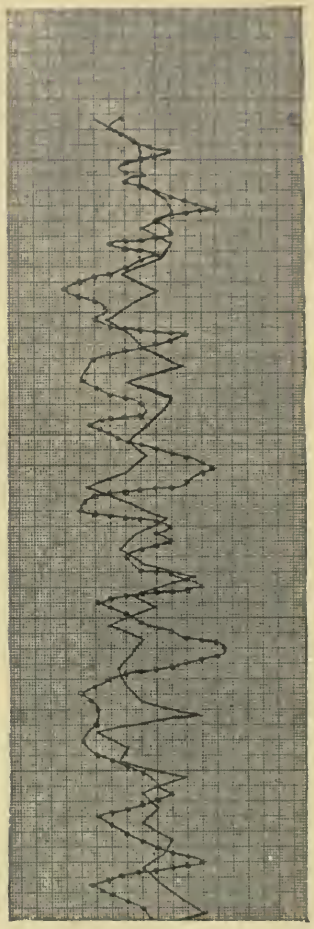


Chart II
Sleep
Barometer





(*b*) Sleep up; barometer down ..... 39 days.
(*c*) Sleep down; barometer up ..... 45 days.
3. Sleep and feeling ..... 162 days.
(*a*) In same general direction ..... 99 days.
(*b*) Sleep up; feeling down ..... 23 days.
(*c*) Sleep down; feeling up ..... 40 days.

4. Work and barometer (Sundays excepted) .133 days.
   (*a*) In same general direction . . . . . 71 days.
   (*b*) Work up; barometer down . . . . . 20 days.
   (*c*) Work down; barometer down . . . . . 42 days.
5. Work and feeling (Sundays excepted) . . . . . 133 days.
   (*a*) In same general direction . . . . . 89 days.
   (*b*) Work up; feeling down . . . . . 14 days.
   (*c*) Work down; feeling up . . . . . 30 days.
6. Barometer and feeling . . . . . 162 days.
   (*a*) In same general direction . . . . . 85 days.
   (*b*) Barometer up; feeling down . . . . . 42 days.
   (*c*) Barometer down; feeling up . . . . . 35 days.
7. Average number of hours of sleep = 7 hrs. 1 min. M.V. = 52 min.
8. Sleep and barometer (child's record) . . . . . 150 days.
   (*a*) In same general direction . . . . . 94 days.
   (*b*) Sleep up; barometer down . . . . . 30 days.
   (*c*) Sleep down; barometer up . . . . . 26 days.
9. Average number of hours of sleep = 12 hrs. M.V. = 40 min.

The curves representing these figures graphically are given on page 428.

The indices of correlation using the 'Whipple' method (cf. *Am. J. Psy.*, Vol. 18, p. 322) found from these figures are as follows:

Sleep and barometer (adult) = − .156.
Sleep and work (adult) = − .031.
Sleep and barometer (child) = + .156.

It was thought that a possible correlation might be found between the variations of sleep and the variations of the barometer on those days when the latter varied over and above an assumed constant variation beyond its average. This constant will here be called 'one.' The adult sleep curve falls below its average six times when the barometer is one or more above its average, but 71 times it is up to or above its average when the barometer is one or more above its average. This gives an idex of correlation of − .951. The child sleep curve

falls below its average 24 times when the barometer is one or more above its average and rises to or above its average 34 times when the barometer is one or more above its average. This gives an index of correlation of — .248.

With regard to the relation between amount of sleep and amount of work, the data may be considered from still another

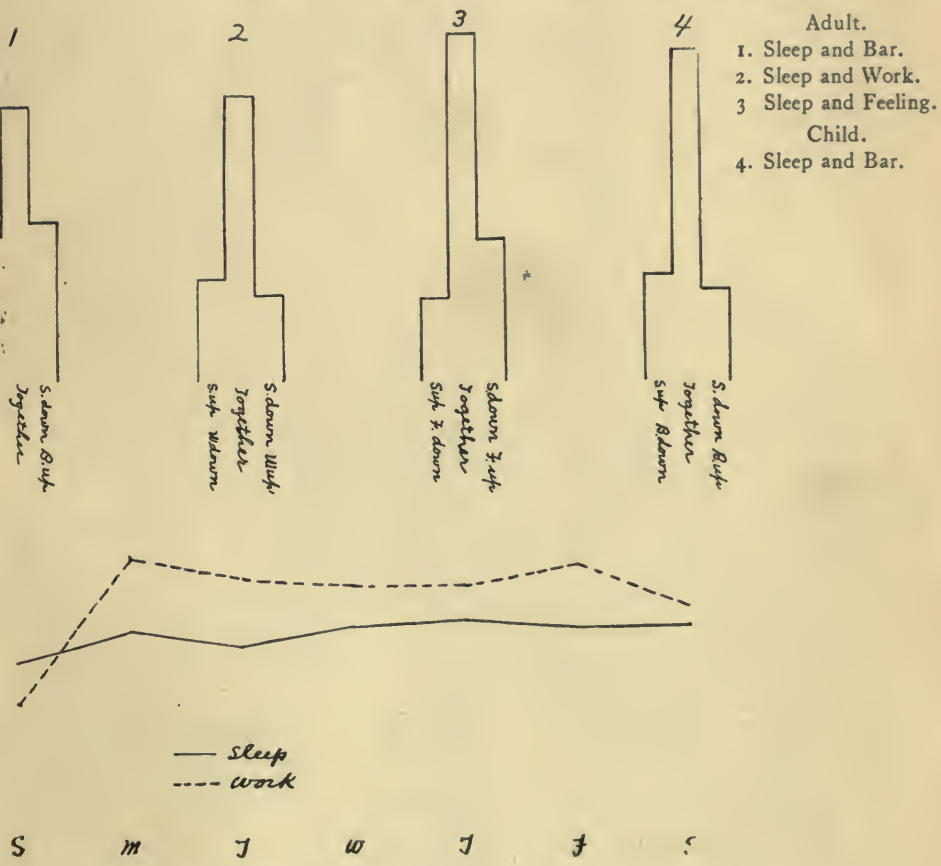


FIG. 1.

point of view. They may be cast into curves representing the average variations of sleep and work for the seven days of the week. When compared these curves show that the work curve is highest on Monday and Friday and lowest on Sunday when it falls to a trifle less than one half its average. The sleep

curve on the other hand falls to its lowest on Sunday night and rises to its highest on Thursday night. (See Fig. 1.) It will be seen therefore that excepting Sunday the sleep and work curves sustain inverse relations, the one rising when the other falls.

All of the attempted correlations with one exception point clearly to one conclusion, namely, that so far as these two persons are concerned there is no direct correlation between the *amount* of sleep and barometric conditions nor between *amount* of sleep, within certain limits, and amount of work. The one exception is presented by the large negative correlation between the adult sleep and barometric conditions when the barometer is considerably higher than its average. This exception is, however, explained in part, at least, by the fact that the notes on daily feeling-tone and quality of sleep (so far as the latter could be determined by the subject's judgment each day) indicate that the correlation between *quality* of sleep and high barometer is not nearly as close as that between the *amount* of sleep and high barometer for the seventy days in question. It is moreover probable when considered in the light of the lack of correlation between deviations from the average of sleep and barometer in case of both adult and child data, that this single correlation, just the opposite to common expectation, would be corrected by more observations.

Of course no general conclusion can be drawn from these limited data; but so far as their indication goes they tend to show that in the evaluation of sleep and its correlation with psycho-physical activities, barometric and environmental conditions, one of the prime considerations is the *quality* of sleep and not its amount. The amount is doubtless highly important when certain limits are transgressed, but *within* these limits we believe the central consideration is *quality* and not *quantity;* that unfavorable environmental conditions and disturbed physiological processes make themselves felt in a change of the quality of sleep before the change in quantity is apparent.

BINDING SECT. SEP 16 1982

BF
1
P7
v.18

Psychological review

PLEASE DO NOT REMOVE
CARDS OR SLIPS FROM THIS POCKET

UNIVERSITY OF TORONTO LIBRARY